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PETROLEUM.*

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I.

It is not without a certain grimness of purpose that a writer nowadays can deliberately take up the subject of Petroleum, and ask anew a fresh interest from a patient but much afflicted public.

The subject itself is in justifiable disrepute. Not only does the mention of it seem to disinter again the memory of the buried losses and troubles of '65, but so much has been written upon it to so small account; so many theories broached in regard to its origin and properties, that stood no breath of question; so much thought and labor upon the subtle fluid, apparently all in vain, that there are few who will not weary of the title and turn the page.

But those who will have the patience to hear him through, the writer hopes, may be repaid; the truth is, that very few substantial facts concerning petroleum have ever been put upon record. We have had narratives of journeys through the country by able reporters; we have had some valuable essays upon it, and all relating to it; but the temporary novelty of the first was soon over, and the few who have dared to grapple with the subject in a scientific way have, for want of the solid comfort that comes from the knowledge of feet and inches and actual results, been compelled to reduce statement to speculation, and fact to theory.

An active professional practice in the oil country for the past eight years enabled the writer to gather much that was useful and valuable, and, finding in his intercourse abroad many serious errors concerning petroleum, it seemed to him that a few simple statements made from an authentic basis, would do much towards relieving the subject from odium, and adding to the stock of general intelligence.

It is not proposed here to consider certain probabilities which have been the subject of much discussion, as, for instance, the "origin of petroleum," or the precise depth of its deposit—questions which, while interesting, are not of present value, or of great importance in comparison with the more pressing inquiries of, Where shall we find it? and how long will it last?

The wisest men in the oil country to-day are those of recent arrival; the most earnest and patient student is the operator of eight years' varied experience, who will frankly tell you, as the result of his labors, that he has learned but little.

Great advances undoubtedly have been made in the machinery of drilling, the management of assured territory, and the handling of the product; but beyond that all

* The series of articles, of which this is the first and introductory, will treat of the natural history, uses, method of transportatation, improved method of boring, refining, testing, etc.

is dark, and for a new field we must grope almost hap-hazard for the valued sand-rock with the long finger of the drill.

The last and perhaps most plausible of the theories for locating new wells, is that of "oil belts." For want of a solid foundation of fact, this, with other promising and carefully-builded suppositions, has fallen to the ground. To supply, in a small degree, some such basis for future thought and speculation, is the object of these essays.

All the oil that has been found in the Eastern United States is included in a strip of land 20 miles in average width, extending from Canada to Tennessee, parallel with the edge of the secondary formation and the Alleghany mountain range.

Concerning this section, Professor Rogers, in his Geological Survey of Pennsylvania, 1836–'57, states as follows:

"The whole of this north-west angle of Pennsylvania is but part of a far larger belt of country, identical with it in geological constitution and in general external features, which stretches north-east into New York, and south-west into Ohio, and borders the south-east side of Lake Erie throughout its entire length. * * * * *

"The mean breadth of the outcrop of the cadent and bergent rocks in Pennsylvania is about 40 miles." * * * * *

"These valleys (*i. e.* the valleys of this section) have evidently been carved by a tremendous rush of waters, and the date of their first excavation was probably coincident with the drainage of the Appalachian Sea, when the mountain ridges and plateaus of Pennsylvania were uplifted, and a portion of the retreating floods swept north into the less elevated region of the Laurentian lakes."

"Subsequently, however, they must have given passage to that almost Continental inundation which, coming from the far North, poured its last expiring currents against the north slopes and table lands of Pennsylvania and Ohio, deepening and opening these south draining valleys, and strewing their beds, and the escarpments facing the lake, with the water-worn fragments of the rocks torn up in its progress."

Professor Newberry, in his admirable report (unfinished) of the Ohio Geological Survey, 1872, states clearly and tersely as follows:

"We have evidence that at one time a broad Continental area filled a large part of the space now occupied by our North America, and was composed of the same rocks that now constitute the Canadian highlands. In process of time this Continent began to sink, and the sea gradually encroached upon its surface; from this sea, in its various advances and retrocessions, our different geological formations have been deposited."

Nine-tenths of the present production is found in that part of the strip referred to, 80 miles in length, known as the Pennsylvania Oil Region.

It is desired particularly to convey to the mind of the reader an accurate impression of the relative size and location of such areas as have produced oil, and which are defined by the spots on Map B, and it will be observed that these spots are isolated and disconnected, with no indication of any continuous belt.

To present this more clearly, it may be stated that out of 2,160 square miles of land in the Pennsylvania oil region, drilled over and accepted as oil territory, but $8\frac{1}{2}$ square miles have produced oil; that is to say, that all the territory that is or has been producing could be contained in $8\frac{1}{2}$ square miles, or 5,440 acres.

An application of these producing spots, *proportionately*, to the great strip on Map A, will show at once how *comparatively infinitesimal* they become.

A careful examination of the relative position of these oil-producing areas fails to suggest any system in their arrangement; a line drawn through three or more of these spots constitutes an oil belt, in the popular sense of the term, but as these lines are drawn in all directions, as the location of the spots readily permits, the value of such an inference may be easily estimated.

It is perhaps safe to assume that at a depth so great as to be to a certain extent a matter of speculation only, and under the whole of the strip or range indicated on Map A, we shall find either the great body or the component materials of this oil. That the animal or vegetable tissues from which it is derived were there deposited during the great northern floods, will probably not be questioned.

What we are searching for to-day, however, is the location of the vent-holes by which this oil reaches within drilling distance from the surface of the earth; whether such vent-hole consists of an open sand-rock, as in Pennsylvania, or of an anticlinal or series of broken rocks, as in West Virginia.

The producing spot in the Pennsylvania oil region is an area containing, at a depth of from 500 to 1,100 ft., a bed of porous open sand-rock, generally formed of pebbles, cemented together, and from 3 to 60 ft. in thickness, the thickest rock giving the best well, and being generally found in the centre, the rock tapering off at the edges.

After the first well is struck, the outline of the spot is readily found by test wells scattering from it.

There is, therefore, within reach of the drill no connected bed of oil-bearing sand-rock dipping southward, but a series of scattered disc-shaped fragments, whose outline and situation, as so far discovered, are indicated by the spots on Map B.

The fact that these scattered beds of porous producing rock are found at a greater depth as we go southward, defines for the present the southern limit of the region; the wells at Brady's Bend, being of the undesirable depth of 1,800 ft., cannot be worked with profit.

To ascertain the extent and regularity of this dip, we take the relative heights of three points above tide water at Philadelphia—Titusville, 1,180.9; Tideoute, 1,099.4; Oil City, 994.7.

On Watson Flats, 35 ft. below the point taken at Titusville, the oil sand-rock is found at 450 ft. The flat at Tideoute is 81.5 ft. lower than Titusville, and the sand-rock is found there at 418 ft., showing that the rocks at the two places are nearly on a level.

Oil City flat is 196.2 ft. lower than Titusville, and the third sand is found there at 475 ft., the distance in a straight line being $14\frac{1}{10}$ miles, and the fall or dip of the sand-rock in that distance 186.2 ft., or something over 13 ft. to the mile.

It will be objected to this calculation that a dip or fall of the bed so nearly parallel with that of the earth's surface, is no dip at all, and in one sense this is true; it is, however, the established practice to compare the record of oil wells, with the sand-rock taken as a level datum line or basis, and for the purpose of utilizing the past work, it is better to be so considered.

Southward, nevertheless, from Oil City, the real dip of the sand-rock is quite rapid. On the river flat near Parker's, the sand is found at 757 ft., the land fall from Oil City being 130 ft., giving a real dip to the rock, in a distance of 24 miles, of 152 ft., or, as calculated above, 282 ft., or 12 ft. to the mile.

The real average dip of the rock from the the earth's surface from Titusville to Parker's is, therefore, 142 ft., or 4 ft. to the mile.

The outer boundaries of the Pennsylvania oil region may be safely considered as defined; all new territory or additional producing spots, will be found within the present limits; indeed, so small and disconnected are some of these producing spots, that even in the heart of the old Pithole district, pierced with a thousand holes, a fresh and untouched rock has lately been found.

It is, therefore, difficult to say when any land is thoroughly tested; a few acres in one corner of a tract may pay for its purchase many times.

The past, present, and prospective future over production of oil, makes it a matter of congratulation that the efforts to solve nature's problem as to its precise whereabouts are unsuccessful.

Were it not so the country that will, in all human probability, supply light for the next century, would be exhausted or ruined in a few years.

While there are about 4,000 producers in the Pennsylvania oil region, the majority of whom will operate at all times and places regardless of the market, the country itself is quietly settling into a few hands. One dozen persons can be named who now own one-fourth of the area of the entire Pennsylvania region, and who are constantly acquiring new land at farm prices.

Land of assured value is not operated by them if the price of oil does not warrant it.

In the consideration of the relative heights of various points in this section in comparison with the water shed or river drainage, the following table of levels will afford food for lively reflection, as being somewhat different from those which a glance at the map would indicate:

Lake Erie, at Cleveland, is 564 ft. above tide water.
Titusvill, Pa., is 1,180.9 " "
Oil City, " 994.7 " "
Mouth of Muskingum River, on Ohio River, is 571 ft. above tide water.
Mouth of Scioto River, on Ohio River, is 474 ft. above tide water.
Ohio River, at Wheeling, is 620 ft. above tide water.

It must be borne in mind that the third sand-rock of the shallow wells is the same as the fifth or even the sixth sand-rock of the deeper wells, all the sand-rocks underlying the coal measures.

In the south part of the region, where six or even more sand-rocks are found on the hills, coal is generally obtained from shallow pits, or short drifts near the surface.

The uncertain days of petroleum are over. No man is to-day a beggar and to-morrow a prince; those who grasp success do so only as the result of most legitimate hard work. The wages of labor are comparatively small, and all branches of the business reduced to the closest system by sharp competition. The next few years will not only find the country in fewer hands, and consequently better controlled, but will disclose some immense fortunes acquired, not by dumb luck, but by hard work, forethought, and patient accumulation.

MAGNETISM OF SHIPS—DEVIATIONS OF COMPASSES FROM LOCAL ATTRACTION.

From the "Nautical Gazette."

The subject of the Magnetism of Ships, their Local Attraction, and the consequent variations of ships' compasses, is one which has received comparatively little scientific attention in America. Indeed, the general intelligence of the commanders of American ships has made each one capable of observing for himself, and with a properly-adjusted compass, and a study of the peculiarities of his vessel, he has formed his own rules, which have answered very well in the management of vessels constructed principally of wood. In England the matter has received much greater attention, especially since the commencement of the construction of iron ships. Various authors have written upon it from time to time—among them Scoresby, Walker, Harris, Gray, Barlow, Airy, Faraday and others; and at Liverpool a Compass Commission has been formed, which has already issued three reports on deviations of the compass and their remedy—finding plenty of cause for the deviations, but unable as yet to suggest an invariable cure for the difficulties encountered.

As the era of iron ships has commenced in this country, and we shall doubtless encounter the same trouble from this cause as has already been and is still being encountered in England, it is well to note the progress of the investigations there, so that we may benefit by them, through avoidance of what are known to be errors, whether of construction or otherwise. In doing this, we shall, of course, be largely indebted for our data to the English writers.

By the term "local attraction," as applied to a ship, we are to understand a certain disturbance of the compass under the influence of the general mass of the vessel, considered magnetically, in virtue of the iron which it contains. The amount of disturbance will materially depend on the direction of the ship's head in respect to the needle, by which the ship's position as a magnet is varied. It is now but too certain that errors of the compass thus produced have led to afflicting cases of shipwreck. It is believed that we owe the first intelligible notice of the local attraction of a ship to Mr. Wales, F.R.S., who accompanied Capt. Cook as the astronomer of his expeditions in 1772–74. Mr. Wales observed, in the English Channel, differences in the azimuth compass of 19 to 25 deg., and afterward similar discrepancies all the way from England to the Cape. The greatest westerly deviations occurred when the ship's head was between N. and E. He was hence led to express his conviction "that variations of the compass, observed with the ship's head in different positions, and even in different parts of the ship, will differ materially." This was certainly the first notice of local attraction scientifically observed, and must not be confounded with the notices of the common action of iron on the compass mentioned by earlier navigators.

In the year 1790, Mr. Downie, master of His Majesty's ship Glory, made an interesting report on this subject, in which he observes "that in all latitudes, at any distance from the magnetic equator, the upper ends of iron bolts acquire an opposite polarity to that of the latitude"—an observation in accordance with an experiment made by Marcel in 1772; so that, by induction, they may attract or repel the north end of the needle, according as the ship is on the north or south side of the equator, thereby causing serious errors in the compass. Admiral Murray and Capt. Penrose, while cruising off the Nap of Norway, observed a point difference in the direction of the compass

when the ship's head was toward or turned from the land.

In 1801 and 1802 this important inquiry received fresh impulse from Capt. Flinders, who, in the course of his voyage of survey to New Holland, also observed differences in the magnetic needle when no other cause was apparent than that of a difference in the direction of the ship's head. When the ship's head was north or south, the needle was not influenced; but when east or west, the difference in the direction of the compass was considerable. Capt. Flinders conceives the magnetic force of the ship's iron to be concentrated into something like a focal point, nearly in the centre of the ship, having the polarity of the hemisphere in which the ship is placed.

These important facts, however, appear to have been lost sight of until Mr. Bains, Master in the Royal Navy, published, in 1817, a valuable little treatise on the variation of the compass; soon after which, in 1819, Prof. Barlow undertook his capital course of experiments, with a view of computing and correcting this source of error. The question of local attraction since this period has received abundant and important verification from the labors of celebrated navigators——Ross, Scoresby, Parry, Franklin, Fitzroy, King, and many others.

The errors liable to arise in the reckoning of a ship's course may, from the local attraction of the ship, be of very serious amount. Let, for example, a vessel be close-hauled upon the port tack, the wind being true north; then, since she sails within six points of the wind, her head will be E.N.E. Supposing, however, that with the ship's head in this direction, the local attraction causes the north pole of the compass to deviate half a point west; then the true direction, E.N.E., will read on the card as E.N.E.$\frac{1}{2}$E., for the E point will then come up half a point. In laying off the course, therefore, on a chart, for the ship's place, she would be reckoned as sailing E. by N.$\frac{1}{2}$N. Suppose the vessel be now put on the opposite or starboard tack, then, having again trimmed within six points of the wind, her head would be W. N.W. Suppose, however, that in this direction of the ship's head, the local attraction now turns the compass needle half a point the other way—that is eastward—which it may; then W.N.W. would read on the card W.N.W. $\frac{1}{2}$W., since the west point would come up, and she would, in keeping the reckoning by compass, be taken as sailing about W. by N.$\frac{1}{2}$N., whereas she would actually be at some point much further northward. Now, if so great a difference may arise upon a comparatively small difference of half a point of the compass, how great must be the evil when the deviation becomes four times that amount! It is, therefore, not at all surprising that very melancholy cases of shipwreck should have so frequently occurred, without any apparent neglect on the part of the officers of the ship. On the 26th of March, 1803, His Majesty's ship Apollo, with a convoy of seventy merchant vessels, sailed out of Cork; and at 3 A. M. on the 2d of April following, the frigate and forty sail of the convoy found themselves on shore on the coast of Portugal, believing at the time that they were 180 miles westward of it. The consequence was a most afflicting shipwreck. Another most remarkable instance is to be found in the wreck of His Majesty's frigate Thetis, which sailed from Rio on the 4th of December, 1830, having on board a million of dollars. The ship's head being southeast by compass, they stood on until the next morning, thinking themselves clear of the land; and, the wind coming free, they tacked, and set studding sails. All at once, after a favorable run, they found the ship against the perpendicular cliff of Cape Frio, running at nine knots. She went stem on to the rock in deep water. Of course, the bowsprit and all the masts were carried overboard, and the ship became a total wreck. The greatest amount of disturbance hitherto observed in vessels built of wood does not appear in certain positions to have far exceeded 20 deg., or about two points; still, a very serious error in the course of a ship. In iron vessels, however, the disturbance may be so great as to render the compass next to useless. In the steamship Shanghai, driven by a screw propeller, the deviation with the ship's head south, as observed by Lilly, amounted in the binnacle compass to 171 deg. 34 min. W., being more than fifteen points.

It is very difficult to determine all the different arrangements in polarity incidental to the iron of a ship, especially in ships of war and iron-built ships, since every piece of iron in the ship may become magnetic by induction, the poles varying as the ship turns into new directions, and changing altogether with the latitude north or south of the equator. The disturbing effect

on the compass also will be different under different angles of inclination, as was completely shown by Capt. Walker, R. N., in a valuable set of experiments on the Recruit, an iron brig. We have, hence, a very intricate problem to solve.

The method of ascertaining the effect of local attraction on the compass, as recommended by Sir W. S. Harris, one of the most intelligent English writers on the subject (to whom we are principally indebted for the foregoing data), is given herewith. Other methods are recommended, as appear in the papers submitted to the Compass Commission, and which will be duly noticed in the course of our review of the subject. The ship, says Harris, must be placed in smooth water, in a slack tide, or in a basin, and must be so circumstanced as to admit of being gradually swung and secured in any direction on the thirty-two points of the compass, by means of warps, mooring buoys, or anchors. The vessel being thus circumstanced, a very distant object is to be selected, and its bearing taken from a convenient station on shore, not liable to any magnetic disturbance. This bearing should be taken with a fine azimuth compass, to be employed as a standard compass of observation, and fixed in a given place on board the ship. Suppose the bearing of the distant object at a station were N. 35 deg. E.; having determined this, we substitute for the compass a theodolite, or the azimuth circle, and adjust it so that the distant object shall read off exactly the same bearing—N. 35 deg. E. The compass is now transferred to the ship, and set upon a firm pillar in the midship line of the quarter-deck. An observer now takes the bearing of the pillar on shore at the same instant that an observer on shore takes the bearing of the pillar on board, which is done by signal. If the ship does not influence the compass, then it is clear that these reverse bearings will coincide in the same line. Thus, if the pillar on shore bore due east from the ship, the pillar on board would be due west from the shore. If this coincidence be not obtained, the difference is the local attraction of the ship. If, for example, while the pillar on board bore due west from the shore, the pillar on shore bore from the ship E. ½N., that is, E. 5 deg. 37 min. 30 sec. N., then the local attraction of the ship, directed in the position in which she happened to be placed, would have been such as to have drawn the north pole of the needle 5 deg. 37 min. 30 sec. toward the east, and this would be the local attraction *for that position* of the ship. In this way, by bringing the ship's head successively upon each of the thirty-two rhombs, and taking what are called cross-bearings, we determine the local attraction or disturbance of the compass for each point of direction. This was the method first pursued by Prof. Barlow; and it is, perhaps, as perfect as any.

The present method pursued in determining the local attraction of the English naval ships is somewhat different. The bearing of some very distant object is first determined by the standard compass from the ship's deck, and for the ship's head, directed upon each point of the compass. The compass is now taken on shore to some convenient spot, and the same distant spot brought to coincide with the observer's eye and the pillar from which this bearing was taken on board, the ship being again swung successively upon the thirty-two points of the compass. If the ship had not disturbed the compass, the bearing should coincide; if not, the difference upon each point is the local attraction. If the object be very distant, the bearings may be simply taken from the two stations, without including the ship, and the difference set down as the local attraction, without any sensible error.

R. Stebbing, of Southampton, has lately invented an extremely available and very valuable method of determining the local attraction of a ship, by which much labor is avoided and time saved. A centre staff, with a flag on it, is set up on some chosen place on shore, and a segment of the magnetic circle, of about a hundred feet radius, described from this point as a centre. Long poles are then set up on this segment, at each 5 deg., and other intermediate shorter poles on each single degree. The line of the magnetic meridian being carefully determined, the true bearing of the centre staff, and its intersection with either of the poles of the segment, are given. With a view to an easy distinction, the poles are either colored differently, or carry small distinguishing flags. The observer on board has now only to take notice what degree the centre staff cuts upon the circle beyond it, and that is the true bearing; the difference, as observed by the compass, is the local attraction.

Other means of correcting local attaction

are: 1. By determining a table of errors; 2. By a compass-card, distorted so as to suit the particular ship; 3. By the introduction of new forces of disturbance, such as will either make known or compensate the disturbing force of the ship.

The method of correction by a table of errors is evidently the first, as it is perhaps the safest, measure we can adopt, and is in all cases indispensable. The ship being swung in the way previously described, the deviations corresponding to the direction of the ship's head are entered in columns of a table opposite each point of the compass, and the correction in steering a particular course applied. Suppose we required to make good a due E.N.E. course, and that, with the ship's head in that direction, the table informs us that the north pole of the needle is drawn by the local attraction of the ship 5 deg. 37 min. toward the west: our course then must be E.N.E.½E. nearly; for that would, in fact, be the direction shown by the card, when the ship's head was in that direction. In effecting a corrected course practically by a table of errors, it will be useful to possess what may be termed an indicator, by which the course to be steered by the standard compass, in order to make good any required true magnetic course, may be found mechanically by inspection.

In correction by distorted card, the ship being swung upon the different points of the compass, a card is marked off, such as on trial will correspond with the true magnetic direction of the ship's head, by which the ship is to be steered. This method has been found very available and satisfactory. The objections are, that the irregular distances of the points of the compass confuse the helmsman, especially in steering ½ and ¼ points, and that it is almost impossible to take an accurate bearing with such a card. Capt. Sparks, however, who has lately obtained a patent for a card of this kind, has ingeniously applied a divided circle to the verge of the compass, by which, when set to the course steered, any bearing may be taken. The idea of a corrected card appears to have been also suggested by Capt. Milne, R. N., in an interesting paper on local attraction, so long ago as the year 1832.

We are indebted to Prof. Barlow for the first attempt ever made to correct the local attraction of a ship by a mass of iron placed in the vicinity of the compass, so as to introduce into the system a new disturbing force, which, acting at a given point, would produce the same effect on the needle as that of the iron of the vessel. In order to understand clearly this kind of correction, we must observe that all the laws which Prof. Barlow had determined, in his researches concerning the operation of regular masses of iron on the needle, he found to attach to irregular masses, whether as a system under the form of detached masses, as in a ship, or under any irregular form. In all cases a close approximation to the action of the system on the needle is arrived at on the supposition that the force proceeds from two centres indefinitely near each other in the general centre of attraction of the mass, and that, in iron bodies, the magnetic force is confined to their surface.

From the first of these principles, confirmed by subsequent experiment, we may infer that the centre of action of all the iron of a ship, and the ideal line joining this centre with the centre of the needle, would be constant in all parts of the world. By the second, we infer that a mere plate of iron may be so placed in this line as to produce an action on the needle equal to that of the ship, so that, the disturbance produced by the plate being found experimentally, the disturbance due to the ship would be known. This principle was first employed by Prof. Barlow.

Since the correcting plate can double the disturbance when placed in a given position in respect to the compass, we may infer that, by changing its position, an opposite point may be found, in which the plate would exactly balance the local attraction by a disturbance in an opposite direction; and such is found to be the case, at least approximately. In applying the plate to the standard compass, either with this or the preceding view, the several bearings for each point must be examined, when two opposite points will be commonly found in which the bearings nearly coincide; the mean of these must be taken as indicating a line of neutrality in the ship. The direction of the line must be noted; and, in some point of this line, the compensator must ultimately be fixed. To determine its exact position, Prof. Barlow has drawn up a general table of local attractions, comprising all possible limits of disturbance for every class of sailing ships in the English Navy built of wood, in which are found the limits of

disturbance applicable to the given vessel. Opposite these limits are two numbers—one representing the distance of the centre of the plate below the pivot of the needle; and the other, its distance from the plumb-line or vertical passing through the pivot of the needle. At this depth and distance in the line of no attraction, and abaft the compass, the compensator will balance all the disturbance arising from the iron of the ship; so that, on swinging the ship, the needle will be found without error. This method has proved eminently successful in practice, although it has, we understand, been discontinued in the English Navy in favor of some other, probably equally good.

Some important practical observations having, in 1835, been made by Captain Johnson, on the steamship Garry Owen, from which it appeared that the ship operated upon a compass-needle placed outside the ship, after the manner of a permanent magnet, Prof. Airy was led, in July, 1838, to undertake an extensive experimental and analytical investigation of the whole subject, with a view to discover such general laws of the magnetic disturbance in iron ships as would enable him to correct the local attraction. The results of his inquiry were published in the Transactions of the Royal Society for 1839. The practical method of effecting his corrections is to swing the ship, as before described; then, by means of two magnets and a mass of iron, to correct the disturbances. The magnets are placed by trial upon some point in one of two lines carefully determined—one parallel to the keel, the other at right angles to the keel; these lines are either traced on the deck, or on the ceiling below the deck-timbers. If the ship's head be north or south, and the transverse magnet be shifted by trial until the compass points correctly, it will be certain then that the force to or from the starboard side is compensated. Similarly, if the ship be swung east or west, the longitudinal magnet is shifted until the compass again points correctly: the force to or from the head is now compensated. As it is requisite, in this operation, to correct the compass simultaneously with the observation of the deviation, the method pursued by Mr. Stebbing, of Southampton, previously noticed, will be found of great value in this case.

Some vessels are more easily managed than others. The compasses in one vessel may require a single magnet only; others require two, with the addition of a box of iron chains. For instance, the Ripon was found to require two magnets and a chain for each compass; the Pottinger had a single magnet only, aided by a chain; and the Ariel's compass was corrected by one magnet only, without any auxiliary aid.

Many objections have been raised, as may be easily imagined, to these methods of compensating the forces, disturbing the compass by the introduction of other disturbing forces—such as the liability of the relations of the magnetic forces to change with change of place and with time; the influence of changes of temperature on the correcting magnets; as also the liability of the magnets themselves to vary in power. Such objections are, of course, inseparable from this kind of investigation; and we can only determine their validity by experience. So far as experience extends, it cannot probably be denied but that the compass, as corrected in iron ships by Prof. Airy's method, has, upon the whole, acted remarkably well. We cannot certainly consider the question to be so definitely determined as to render all further observation unnecessary; it is very important, as stated by Prof. Airy, to subject the vessel from time to time to further examination, and carefully note all the changes which are liable to occur. There is little doubt but that compasses corrected by permanent magnets are affected by time and by geographical position; but still not to such an extent as is likely to lead to any very sensible error, or an error which may not be provided against. Ships, however, destined for long voyages, should still depend materially upon a table of errors, registered for a standard-compass, whatever other method of correction of the compass be resorted to. Corrected cards are decidedly useful, especially in iron ships, and may be employed with advantage, in conjunction with other means, to determine the true magnetic course.

The following are a few important facts, as deduced by Mr. Stebbing from his experience of iron ships: 1. A compass may be very true on one or several points, and greatly disturbed on others. 2. The errors of one ship are no guide to the errors of another. 3. The errors are least toward the middle of the vessel. 4. Every iron ship is a magnet in itself: some have the north pole aft, and some the south. The magnetic axis is frequently determined diagonally through the ship. 5. There are

in all iron ships two points, either opposite or nearly so, at which there is no error; there are two other points where the error is the greatest. An error will sometimes not alter three degrees in a range of five points, but may then change thirty degrees in the next five points.

So much of this important subject as is indicated in the foregoing review was determined in England as early as 1852. In 1855 the first report of the Liverpool Compass Committee was made to the Board of Trade. It was mainly confined to a description of the mode of operations of the Committee, and embodied a series of questions in form, to be propounded to the commanders of all vessels sailing out of British ports—such, for instance, as the following: What is the nature and amount of deviation of the compass (in either wooden or iron ships)? Does it differ, and to what extent, in the same ship at different times, and under what circumstances? What changes, transient or permanent, are caused by cargo, concussion, straining, or change of geographical position? How may compass errors be obviated, and what practical rules or remedies are most applicable? These main questions were minutely subdivided, so as to insure a complete record of all essential facts bearing on the investigation, and observations with an elevated compass were urged as especially desirable.

The second report of the Committee was made in 1856. It contained the results of observations and experiments made in accordance with the plan set forth in the first report, but was not so satisfactory in all respects as the Committee could have desired. The records, however, no longer allowed a doubt as to the connection which exists between the direction of a ship's magnetism and her position when upon the building slip. In all the ships which had been examined, the north end of the compass needle invariably deviated toward that part of the ship which was furthest from the north while she was building, if the compass was placed in a central position, and free from the influence of individual masses of iron. This was determined after numerous experiments. The Committee also strongly recommended the use of the "dumb-card," or "azimuth card without a needle," finding the magnetism or local attraction of a ship.

The third and most important report of the Committee was made in 1860, covering the preceding four years. The chief points insisted upon were announced to be that the magnetism of iron ships is distributed according to precise and well-determined laws.

That a definite magnetic character is impressed upon every iron ship while on the building slip, and is never afterwards entirely lost.

That a considerable reduction takes place in the magnetism of an iron ship on first changing her position after launching; but afterwards that any permanent change in its direction or amount is a slow and gradual process.

That this original magnetism of an iron ship is constantly subject to small fluctuations, from change of position, arising from new magnetic inductions.

That the compass errors occasioned by the more permanent part of a ship's magnetism may be successfully compensated, and that this compensation equalizes the directive power of the compass needle on the several courses on which a ship may be placed.

These deductions, it must be explained, were founded upon observations and experiments made in port. The principal topics which remained for discussion and inquiry were, the effect of heeling on the compasses of iron ships, and the changes which occur on change of magnetic latitude.

In considering the compass deviations from heeling, it was early a subject of remark that the evidence before the Committee generally indicated an attraction of the north end of the compass needle to the high or weather side of the ship, in whatever direction she listed, while published experiments on this subject, made on board the Bloodhound, by direction of the Admiralty, showed, on the contrary, the deviation of the needle toward the lee side, and of comparatively small amount. The reports of merchant captains were, in consequence, received with some degree of caution, until they eventually became so numerous and so consistent as to demand the immediate action of the Committee. It was decided, therefore, that a large iron ship should be heeled and swung. As certain observations made by Captain Leitch, on board the steamship City of Baltimore, showed that this ship would be a desirable one for the purpose, she was placed at the disposal of the Committee. The results proved conclusively that the errors from heeling were so

large as to very seriously affect the safe navigation of the ship, when not allowed for. Thus, the azimuth compass of this vessel, which is placed about 4 ft. above the deck house, nearly 11 ft. above the deck, and about 30 ft. before the mizzen mast, when the ship's head was placed north, correct magnetic, showed a deviation to the west of 7 deg. 30 min.; when she was heeled 10 deg. to starboard, the deviation was 25 deg. 30 min. to the west; and when she was heeled 10 deg. to port, the deviation was 15 deg. 3 min. to the east; showing a difference of 41 deg., due to heeling alone, without in any way changing the direction of the ship's head; or an average change of 2 deg. of deviation for each degree of heel! This was about the maximum change for the azimuth compass, for it was found that when the ship's head was east or west, by compass, there was practically no change of deviation from heeling, and that the change gradually increased as the ship's head approached north or south by compass.

The results of these experiments were thought so very remarkable, that the deputy chairman of the Committee, who was then having a large iron ship built in Liverpool, with her head toward the east, determined that this vessel, the Aphrodita, should also be heeled and swung before she went to sea. A short time afterward, the owners of another large ship, the Simla, then building on the opposite bank of the Mersey, with her head toward the west, consented to have her heeled for similar experiments. Both these ships, like the City of Baltimore, showed very large deviations from heeling, while the general direction of their magnetism corresponded with the opposite direction in which they were built; the chief attraction of the compass needle in the Aphrodita being toward the starboard side, and in the Simla toward the port side. But both ships corresponded with the City of Baltimore in attracting the north end of the compass needle to the high side when they were heeled over.

Had the heeling experiments of the Compass Committee closed here, there would undoubtedly have appeared some incongruity between them and the Admiralty experiments before noted. For, though it would be easy for one conversant with the magnetism of iron ships to indicate what distribution of a ship's magnetic force would to them account for the observed phenomena, it is felt that the majority of the commanders of iron ships could not be expected to possess this information, and that they may be excused for holding the opinion now prevalent among them, that there is something wayward and uncertain, not to say marvellous, in the deviations of the compass which arise from heeling. Fortunately, a fourth vessel of the same large class as the Simla and Aphrodita was afterwards heeled, at the request of the Secretary, and proved, as it were, a connecting link between the two sets of experiments. The results obtained in this case demonstrated practically what had previously been suggested by theory. A few days before this ship—the Slieve Donard—was swung, some preliminary experiments were made with the dipping-needle and vibration-compass, to ascertain suitable positions for the compass. These indicated so little vertical magnetic force in the after part of the vessel, compared with those made on board the other ships, that it was at once suspected that the Slieve Donard would show very small error from heeling, and that she could not have been built head nearly east, as had been supposed, from the known position of the building yard. On going to the yard, it was found that, owing to the great length of the ship, the ways on which she was built had been placed obliquely, so that the direction of her head while building was nearly three points south of east! The smallness of the observed dip was thus satisfactorily accounted for, and the inference which had already been drawn in respect to the deviations to be expected from heeling this ship was proportionately strengthened. In these experiments the standard compasses were chosen, and placed in carefully-selected positions, free from the disturbances of particular masses of iron, and at least 4 ft. above the deck; in the cases of the Aphrodita, Simla and City of Baltimore, from 11 to 12 ft. above the deck.

The most careful observation tended to show that there was a general consistency in the results obtained. Thus, in ships built with their heads more or less to the north, it was proved that there is a strong magnetic attraction of the opposite name below the compasses in all the after part of the ship, the effect of which is to increase the deviations observed with the ship upright as the ship is heeled in one direction, and to decrease them when the ship is heeled in the opposite direction, the result being equivalent to an attraction of the north end of

the compass needle to the high side of the ship. In ships built with heads more or less to the south, as there will be the contrary magnetic attraction below the compass, the reverse effect from heeling is exhibited. It remains to be explained why the deviations from heeling in ships built head to south are so much smaller than in ships built head to north in these northern latitudes. The principal, but not the only cause of this, is believed to be the induced vertical magnetism of the deck beams, and other similarly placed pieces of iron, as the ship heels over. In this latitude the upper ends of the beams instantly acquire southern polarity, and, in combination with the natural south polarity of the after part of the vessel built head to north, jointly produce the deviation of the needle to the high side; but in ships built head to south, there is only the difference, instead of the sum of these two forces. This being the case, it is apparent that there may be some ships in which these two forces will cancel each other in this latitude, and that those ships must necessarily be built in north latitude, with their heads somewhere between south and west and south and east. The Slieve Donard indicated more closely those limits for ships of her class built in England, and places them near S.W. and S.W. by W., or S. E. by E. Further, as these deviations from heeling, like ordinary deviations, are partly due to the more permanent and partly to the induced magnetism of the ship, it is evident that they must change with change of magnetic latitude, and that in high south latitudes the reverse phenomena must be expected to obtain; namely, that ships built here, with heads more or less to the north, will have comparatively small deviations from heeling in high south latitudes, and that ships built head to south will have larger deviations, and of the contrary name, to those exhibited in north latitude.

The *general* effects of heeling on the deviation of the compass in iron ships may be stated as follows:

1. The maximum deviations from heeling will occur in iron ships when they are upon or near northerly or southerly compass courses. They will decrease as the ship approaches easterly or westerly compass courses. There will practically be no deviation from heeling when the ship's head by compass is east or west.

2. In ships built in this latitude, with their heads toward any point between W.S.W. and E.S.E. by way of north, the north end of the compass needle deviates toward the high or weather side of the ship, and, other things being the same, to the greatest extent in ships built with head at or near north. In these ships the deviation from heeling will increase as the ship attains greater north magnetic latitude, and will decrease as the ships go toward south magnetic latitude. In some cases the deviation from heeling in these ships may disappear in high south magnetic latitude, or even change its name.

3. In ships built with head between S.W. and S.E. by way of south, the deviation from heeling will usually be small in north magnetic latitudes, and the north end of the needle will generally be attracted toward the low side of the ship. The deviation from heeling in these ships will increase as they approach south magnetic latitude.

4. In ships built in positions intermediate to those named in sections 2 and 3, the deviations will usually be small in ordinary north latitudes, and are not likely to be very large in ordinary south latitudes.

5. To test whether a ship's compasses will be affected by heeling or not, she should be listed over while her head is at or near north or south, both to port and to starboard, as in some cases there may be a moderately large deviation from heeling as the ship inclines in one direction, and very little, or even none, when she is heeled the opposite way. The trial should be made with head north, if the ship was built with head northerly, and with head south, if built with head southerly. This precaution will be most necessary when the compass is near the stern. The deviation from heeling on the intermediate points may be approximately found by multiplying the maximum deviation from heeling into the sine of the angular distance of the point in question from east to west.

6. An approximate idea of the extent to which a ship's compasses will be affected by heeling, may be obtained by dipping-needle and vibration experiments, without actually heeling or swinging a ship, by persons who have acquired previously some experience in these experiments and in the magnetism of iron ships; but until further data on this subject are accumulated, the most satisfactory course is to swing every new iron ship with a list to port and to starboard, as well as upon an even beam.

The foregoing remarks, it must be re-

membered, are intended to apply to uncompensated standard compasses, placed in the ordinary positions, and have relation only to the *extra* deviation which is caused by heeling. Other results are also possible, as, for example, in ships without iron deck-beams. It is also possible for the deviation from heeling to be in the same direction, whether a ship inclines to port or starboard; but in any such case it is believed that the deviation from heeling in one direction, if not in both, must necessarily be very small in amount; so small that it could not practically affect the navigation of an iron ship. The compasses in these experiments, which were compensated in the usual manner, with magnets placed below the level of the card, have shown comparatively small errors from heeling.

A report of deviations from heeling observed on board the Sharpshooter was furnished to the Committee by Mr. Evans, of the Admiralty Compass Department. This vessel is reported to have had no iron deck-beams; but nevertheless the quadrantal deviation was comparatively large; she is regarded, therefore, as affording an apt illustration of the magnetism of the floors and bottom plates of a ship being conducted up the sides. This effect was also very apparent in experiments made on board the Accrington, which experiments also induced the belief that an important proportion of the resultant magnetism is due to the Earth's induction. The interesting and important question here arises: How much of a ship's polar compass deviation is due to that magnetism which is, as it were, hammered into her while she is being built, and how much to the vertical induction from the earth which belongs to the ship's geographical position at the time of the experiment?

Heeling experiments may be expected to afford approximate values for the sub-permanent and induced vertical magnetism of the several ships, but the attempt to estimate them has not proved satisfactory. The formation of a general rule in this regard may be considered an impossibility, as the conditions relating to any two ships can never be exactly alike, even though of the same size and occupying the same position in the building slip. At present the only direct experiments before the public upon the changes which occur in the vertical magnetism of an iron ship on change of geographical position, are those made by Dr. Scoresby during his voyage to Australia and back, on board the Royal Charter; but these are regarded as unsatisfactory by the Compass Committee, by whom they are termed very rough approximations to the elements of the vertical magnetism of the ship. It will be necessary to make complete vertical and corresponding horizontal force experiments at several stations on board a ship, and with her head as frequently as possible in opposite directions, before anything like a satisfactory estimate of the components of a ship's vertical magnetism, and the changes which occur during a sea voyage, can be obtained.

At the time the Compass Committee commenced their inquiry, doubts were frequently expressed by persons who were thought to be well-informed on the subject, as to the correctness of the reports made by the captains of some iron ships, of the large deviations which they had experienced in south latitudes; and this incredulity was certainly encouraged by some interested compass-adjusters, as well as by some ship-owners, who seemed to fear that iron ships would thereby acquire a bad name. Trustworthy evidence was, however, very soon produced of the extent of the deviations which arise from the change of geographical position. One example will be sufficient. The steering compass of a new iron ship, the Harvest Home, built with her head to the eastward, was carefully adjusted at Liverpool before she proceeded to the East Indies. On the arrival of the ship at Calcutta, the captain wrote to his owners as follows: "My compass acted very well until I entered the 20th deg. of south latitude, when the binnacle compass commenced to deviate considerably. I found the greatest amount of deviation in 39 deg. south, and from 25 deg. to 45 deg. east longitude, the binnacle compass then being 7½ points out. Unfortunately, I had the standard-compass broken to pieces with a heavy sea. I afterward placed the azimuth compass on that part of the house where no deviation exists. It has ever since given me entire satisfaction.

This and similar records were sufficient to show that extreme changes occurred in high south latitude, and when the ship was on easterly and westerly courses, clearly proving that the change was in the fore-and-aft magnetism of the ship, and chiefly due to inductive magnetism. The Committee complain, however, of an almost total absence of recorded azimuths or amplitudes,

making many of the logs very nearly worthless for the purposes of the investigation. The mode of navigating many iron ships may be described by the following quotation from the journal of Dr. Scoresby's voyage: "The captains of ships proceeding from Australia to the eastward appear generally to be guided in their allowances on the dead reckoning by the agreement or differences discovered from day to day between the positions of the ship, derived from suppositious allowances for compass errors, and the positions indicated by celestial observations. If they find themselves considerably to the southward of their reckoning on any particular day, they allow more easterly variation or deviation proportionally, or the converse. This practice while the sun can be regularly seen, and while the course lies wide of any land or rocks, does very well; but when, as is not unfrequently the case, celestial observations may not be attainable for days together, or when the ship's course may lie pretty nearly in the direction of land, and especially when a change of wind may oblige a change in the course, and so, in the ship's deviation, a reliance upon such inverse processes may obviously prove not only very embarrassing, but in heavy weather, with fog and darkness, not a little hazardous!" All of which may be taken as gospel on the subject. The advocates of this "inverse process," which scarcely deserves the name of navigation, have, no doubt, been forced to adopt it, to some extent, through the absence of proper variation charts.

Fortunately a few of the logs placed at the disposal of the Committee were very full and satisfactory, and were chiefly employed to ascertain the proportion which exists in the different ships between the sub-permanent and inductive magnetism, with a view to the practical application of the results to the permanent correction of the compasses of iron ships, and more especially to the correction of the steering compass in its ordinary position. Variation charts were found useful in ascertaining what portion of the observed compass error was due to deviation and what to variation, as well as how far the former was affected by heeling—which would be almost impossible by the "inverse process." The Committee for this purpose strongly recommended Mr. Evans' Admiralty Chart of the World, which has been quite recently republished by our Government.

The theory upon which the examination was made is probably the most simple that could be adopted, and is believed to be sufficiently accurate. In fact, the results obtained will indicate of themselves the extent to which other considerations should be introduced. The theory is that the deviations observed when the ship is upon or near to the cardinal points of the compass will give very approximate values for the ship's fore-and-aft and transverse magnetism; that where observations for deviation are made on several points near to the cardinal points, a curve drawn as nearly as possible through a projection of these deviations to the cardinal point will satisfactorily represent an observation taken on the cardinal point; that the deviation observed will be partly due to permanent and partly to induced magnetism; that the part due to the former will vary in the inverse ratio of the earth's horizontal force at the places of observation, and that the part due to the latter will vary as the tangent of the corresponding dip.

It will be instructive here to note a few of the leading points of these logs, and the deductions of the Committee regarding compensation of the compasses, where such deductions have been made.

The Advance, an iron ship of about 600 tons register, was built in Liverpool, with her head a few degrees to the south of east. Her steering and standard-compasses were both compensated; but, after she had been at sea a few days, her commander, observing that the compasses did not agree with each other, removed the magnets altogether from the standard-compass, but did not interfere with the compensation of the steering compass. In south magnetic latitude the difference between the compasses increased, until it was sometimes four or five points, and on one occasion, in 62 deg. south dip, a difference of about eight points was recorded. The standard-compass during the voyage showed only a very small change in the ship's fore-and-aft magnetism. The deviations from the transverse magnetism varied from 7 deg. to 26 deg., nearly the whole due to permanent magnetism. Had a transverse magnet been applied to this compass at Liverpool, sufficient to have produced a minus deviation of 20 deg., it would have been slightly under-compensated during the first part of the outward voyage, and, through a reduction in the ship's transverse magnetism, would have been slightly over-compensated on the homeward voyage, and the maximum polar deviation would probably never

have exceeded half a point. The standard compass on the second voyage of this ship was placed further forward, so as to be between the mizzen and main masts, and immediately over the break of the poop, about 47 ft. from the rudder head, and above an iron bulkhead. It was 7 ft. 4 in. above the poop deck, and 11 ft. 2 in. above the main deck; a position in which the deviations were found to be very small. The magnets at the steering compass were not moved. Whether they had decreased in power, as seems probable, is not known, nor whether there was any deviation observed in port.

Before the Advance made her third voyage, an upright bar of iron, about 8 ft. long, 2½ in. wide, and 1 in. thick, was fixed at the back of the cabin and before the compass. It was found too weak for the purpose intended, and the fore-and-aft magnets had to be shifted a little further from the compass. On her return to Liverpool she lay in dock with her head to the north, so that there was no opportunity of obtaining the fore-and-aft error of this compass; but the fore-and-aft magnets were taken on shore, and placed in the same relative position as when on board, with a trial compass, when they were found to produce an error of 10 deg. There seems little doubt that a vertical bar of soft iron introduced before this compass, so as to produce at Liverpool a deviation toward the ship's head of 15 deg. 45 min., and a permanent magnet placed on the deck, with the north pole toward the bow, so as to neutralize the deviation caused by the bar, would satisfactorily correct this compass for all latitudes. It should be stated that on the last two voyages the advance had on board considerable iron as cargo. The sea errors of her standard compass, however, were so small as scarcely to require any attempt at artificial correction. A transverse magnet with its north end to starboard, sufficient to produce at Liverpool a deviation of 7 deg., would certainly be sufficient for all practical purposes. The residual errors and the errors from dealing would then be too small to give the navigator any anxiety, when unable for a day or two to make any observations for the correction of the compass.

The Astræa, a three-masted schooner of about 400 tons, was built on the Clyde with her head to the west of south. Her first voyage was to the West Indies, and she has since made several voyages to the island of Mauritius and to Calcutta. Her captain, at the request of the Compass Committee, repeatedly swung his ship at sea, and the details furnished by him present one of the most complete records received of the changes which occur in the magnetism of iron ships. Her steering compass is placed over the cabin skylight. According to the deviation table given by the compass-adjuster before leaving the Clyde, the steering compass had a deviation of 2½ deg. west when the ship's head was east, and no error with her head at west. The attraction to port was rather over-compensated by a transverse magnet, the table showing no error with the ship's head north, and with the ship's head south a deviation of 4 deg. west. As the deviations on the card are all westerly, there is evidently an index error of some sort. After allowing for this, the resultant attraction to starboard may be taken at 1 deg. When examined in Liverpool, after her return from the West Indies, it was about 15½ deg., showing a considerable reduction in the ship's magnetism. On removing the transverse magnet, the attraction to starboard was 8½ deg. Supposing the magnet to have retained its power, the original attraction to starboard will be represented by a deviation of about 23 deg. There was also found an attraction toward the head of 8½ deg., indicating a reduction of the same extent in the ship's fore-and-aft magnetism. On her return from Mauritius, the deviation toward the bow had increased to 11 deg. The deviations of this compass rapidly increased as the ship went south, especially when her head was east or west; and the captain, observing that the magnet caused an increase of deviation on some other courses, removed it from the compass altogether. Besides the steering compass, there was a standard-compass, placed near the mizzen mast, but it does not appear to have acted well. The deviations of the standard-compass are reported to have been frequently of the opposite name to those of the steering-compass, so that in the southern hemisphere there was sometimes a difference of several points between them; thus, on one occasion a difference of 8½ points is recorded, and seven days later a difference of 10 points. Experiments made on board this vessel fully confirm the assertion which has been so frequently made by the commanders of

other iron ships, that in certain places and positions of the ship the steering compass has been found to be entirely destitute of directive power. This would, of course, occur when the ship's force and earth's force were nearly the same, and acting in opposite directions.

These instances are sufficient for our present purpose. It was made evident from these and other experiments that a portion of the ship's changing magnetism does not change immediately on change of magnetic latitude, or that a portion of the magnetism treated as permanent is only so under certain conditions. The fluctuations in the value of the more permanent part of the magnetism of the Advance are smaller than in the Astræa, even when, in the latter ship, only the observations made in the southern hemisphere are compared with those made in Liverpool; but in both cases the fluctuations are within limits which make compensation by fixed magnets and an ordinary bar of vertical iron in all probability practically efficient. If the vertical bar proposed to be placed before the compass descended to the keel, and formed part of the structure of the ship, it seems pretty certain that the changes in its magnetism would correspond so closely with the changes occurring in the stern-post and other iron aft of the steering compass, that the fluctuations alluded to would altogether disappear. There are reasons for supposing that these fluctuations attain their maximum in ships built head to the south.

The examination of a great many logs tended to convince the Committee that a large proportion of the errors of the steering compass was due to vertical induction, and that they could be in great measure compensated by the vertical bar placed immediately before the binnacle. Repeated experiments confirmed this conviction. In no instance has the effect of the iron bar been in excess; on the contrary, it has been found that either a large bar, or the placing of the compass nearer to the bar, would have been an improvement. If the bar were fixed while the vessel was building, so as to be subject to the same concussions as the stern-post, etc., the compensation may be expected to be more perfect.

Experiments made by the Committee on the compensation of that portion of the magnetism of iron ships which produces quadrantal deviation, led to the following, among numerous conclusions:

1. Quadrantal deviation may be large in amount in a small ship, and small in a large ship, and the reverse.

2. Generally, it is larger in amount in the middle of a large ship than in positions nearer to the ends.

3. It decreases rapidly as the compass is raised above the deck, and at a certain height above it may change from plus to minus.

4. In the usual positions for a compass the quadrantal deviation is almost invariably plus, but it may be minus in a compass placed over a hatchway or skylight, or on the bridge between the paddles of a paddle-wheel steamer, or, as before stated, at some height above the deck, as on a mast.

The Committee, however, do not profess to give any explanation why the quadrantal deviation is so much greater in some ships than in others, or why it should be so much greater in some quadrants of the compass than in the adjacent ones in the same vessel.

The Committee also briefly discuss the relative qualities of cast and wrought iron, and finally come to the conclusion that "undoubtedly the best practical corrective for errors of the compass of all kinds is to be found in a competent and careful captain."

At the present time, there are 800 Pullman palace cars arriving at and departing from Chicago every day. The amount of money expended in their manufacture ranges from about $10,000 to $30,000 for each car, the average cost being about $16,500. The amount of capital, therefore, invested in the Pullman cars which accommodate the business of Chicago, amounts to about $1,300,000. It is reported that the company has purchased a large piece of ground somewhere in the suburbs of the city, on which they will shortly erect large shops for the manufacture of their cars.

Work was commenced on the Hoosic Tunnel December 2, 1868. The total cost, according to contract, is to be $4,600,000. The average progress made per month is 385 ft.; 8,528 ft. have been excavated, leaving 7,165 yet to bore. The State has been offered $240,000 a year for the use of the road.

THE COAL MINES OF SWEDEN.

From "Engineering."

The following translation, from the Swedish, by Mr. C. P. Sandberg, is from a letter recently addressed to the "Aftonbladet," by Mr. S. Nilsjon, the celebrated Professor of Natural History and Archæology, upon the important subject of the recently discovered coal deposits of Skane.

Professor Nilsjon says:—The reason why I have, for so long a time, taken an animated part in everything concerning the Skanian coal fields, is not entirely due to a scientific interest in the investigation of the numerous and, in a geological sense, remarkable strata containing petrifications, which occur in this province, but also and particularly because I consider the existence of coal in Skane may avert the danger now threatening some districts of the province, which may be rendered all but inhabitable at a not so very distant period. The peat bogs in many places are nearly exhausted; the forests are yearly decreasing; foreign coal is continually rising in price; and our forest laws permit Danes and other foreigners here to buy up large and small timber, and immediately to fell and export it, so that where the traveller in Skane, a few years ago, journeyed through dense forests, there remain at present nothing but the stumps; all the wood has been transported across the Sound.

Such destruction of forests and carrying away of their produce from the country is prohibited in Denmark, and I do not know whether it is permitted in any other civilized country but this. If the existing state of things is to continue, it will not be difficult to comprehend that in a few generations at the longest, there will arise a great dearth of fuel in various parts of this populous province, unless a sufficiency of native coal can be found to compensate for the exhausted peat bogs and the forests so carelessly destroyed. It is on this account that I, for my part, consider the investigation now being carried on for the discovery of coal, and which is said to be organized on a new and excellent method which reflects great credit on the administration, to be of the greatest importance, for this province in particular, and for the whole of our common country in general.

I consider myself, in consequence, called upon to make a short statement of what is within my knowledge concerning the extent of the Skanian coal fields, and the formations of rock that are related to the coal-bearing strata.

The first of the formations belonging to this class is the lias-sandstone, which crops up in different shapes at various places within the province. I will mention, to begin with, the sandstone appearing at Höör.

In the year 1819 I happened for the first time to discover at this place some curious impressions of plants, which experts considered to have been the growth of a tropical climate. I made annual excursions to this spot, and found there, as well as in the clay schist from the coal mine at Höganäs, many fresh forms of vegetable growth. These were described and delineated in the "Transactions of the Academy of Sciences," and by that means became known in foreign countries.

In the year 1824 there arrived at Lund the Professors Brougniart, father and son, from Paris, the former a mineralogist and geologist, the latter a botanist. They were met here by Professor Berzelius, from Stockholm. After having inspected the collections here in Lund, and the younger Brougniart having copied several fossil plants, we proceeded to Höör, where all the cuttings in the rocks were searched, and some objects of interest obtained. Afterwards we went down the country to Cimbrishamn, where a remarkable piece of sandstone was investigated. Everywhere the formations of rock encountered were defined by the elder Brougniart, according to the system which he published a few years afterwards in his work, "Tableau de Terrians, etc.," Paris, 1829. Afterwards we returned to Lund, and thence to Höganäs, where the coal-bearing strata and the schist, with impressions of vegetable forms, were examined. In reference to this journey, the younger Mr. Brougniart published during the same year his "Observations sur les végéteaux fossiles dans les grés de Höör," and he afterwards included this treatise in his "Prodrome d'une histoire des végéteaux fossiles (1828)."

Two years subsequently, the administration of Höganäs colliery, sitting in Stockholm, resolved, on a proposal to that effect, brought forward by the then Councillor of

State, Count B. B. von Platen, to engage an English mining engineer, in order to furnish the administration with information as to the Skanian coals. Mr. Robert Bald, being applied to, undertook to come here, and when it then became a question who would be willing to accompany Mr. Bald on his tour of investigation through the province, I was proposed for this office by Berzelius, and met Mr. Bald by appointment at the estate of Säbyholm, belonging to the General-Baron Cederström, where Count Platen and others had already arrived. At Helsingborg, Mr. Bald and Mr. D., of Uhr, joined, and thence we proceeded to Höganäs, where Mr. Bald descended into the mine and investigated the circumstances in all their bearings. I think I remember that in a shaft which was opened, the bed rock had been reached, and I obtained a piece of it with galena, exactly like that at Gladoax. In the bay off Jonstorp I also saw, at low water, a rock formation which appeared to me to be primeval.

After Höganäs we visited Helsingborg, where coal had been dredged out of the sea, to the north of the town. Thence we proceeded to Kropp, Raus, Mörarp, Bjuf, Bosarp, Billesholm, Wallakra. At all these places small quantities of coal had been got at, or some coal mine, formerly worked, had been closed up, as for instance at Bosarp, where poor women went about picking up bits of coal. I remember seeing scattered pieces of the fire-clay, as well as of a liver-colored, iron mixed clay (thoneisenstein), which had been dug up along with the coal. We also visited Höör and the country round about, and subsequently paid a visit to the Ifsjö Lake, where we examined the peculiar chalk formation of the whole of this neighborhood, consisting solely of more or less broken shells, mussels, and corals, which had lived in numbers of millions of millions in the sea covering this district in remote ages past. Sometimes bones were found of vertebrate animals, in particular of fishes and gigantic amphibiæ, such as the Mosasaurus, and, as I have reason to believe, likewise the Plesiosaurus.

We then moved more towards the south. In the southern portion of Färs hundred, there is a breeding farm called Tagra, belonging to one of the Counts Piper. Below the farm-yard there is a morass, with a pretty large sheet of open water, in which jets of spring water are seen to bubble up. This pond is remarkable by reason of two rivulets issuing from the same, one towards the south giving rise to the Allevad river, which forms the boundary between the hundreds of Ingelsta and Herresta, and discharges itself into the Baltic, below Köpinge; the other rivulet runs westward, and forms one of the sources of the considerable river which courses between the hundreds of Färs and Torna, and finally empties its waters into the Sound, below Löddeköpinge. The Tagra morass consequently lies on the summit of the land in this part of the province. The water is remarkable on account of the excessive quantity of chalk which it contains in solution, and which makes deposits on the reeds or stems of grass which grow in the pond. Any rushes or other objects which may have been thrown into it will be covered by a thick incrustation of chalk. Down towards the valley of the Allevad river there are to be found mighty layers of such tufa-chalk, and if my memory serves me right, the Benesta church is built of this material. I also remember having collected, when a student, pieces of tufa with many impressions of plants (probably from this place) among the ruins of one of the monastic churches at Lund.

At Ramsasa there is a kind of sandstone which, although not quite sure, I consider to be closely allied to the lias. One kind of sandstone in that neighborhood is cut into convenient shapes and sold as whetting-stones, just like the sandstone north of Helsingborg, which it resembles, and which appears to me to belong to the coal formation. Further down, in the western river side, at the Svenstorp Mill, is a layer of chalk formation, which a geologist, since dead, considered to be molass, but which in reality, as before stated, belongs to the chalk formation. Still further down, at the Mill of Kopinge, there appears a thick layer of gravel and sand, in which is often to be found whole specimens, though without the shell, of the gigantic conch Ammonites Stobæi, which is described and delineated in "Petrificata Suecana," and of which specimens sometimes are found 5 ft. and more in circumference. To the same formation belongs likewise a kind of hard sandstone found at this place, in which have been discovered fragments of amphibious animals described in the "Transactions of the Society of Science." From here we proceeded down to the sea coast to the foot of Käseberga Hills, where I was told coal had been found. Arrived at the shore, where these immense

sand-hills terminate abruptly, we discovered on the margin of the sea pieces of coal strewn about, which at the first glance Mr. Bald thought were English, and the remnants of some coal-laden vessel which had perished on the coast; but a blacksmith who lived in the neighborhood, and came down to us, related that he had for many years worked his forge with coals which he had at this place fished up from the bottom of the sea, and that at low water he could see the coal bed; he also assured us that he had followed this bed, extending below the surface of the sea, underneath the sand-hill.

It is evident that the coal formation of Bornholm is a continuance of the Skanian; it runs in the same direction, with similar and in the same manner accompanying strata of sandstone, clay, iron-clay, as with us. It would appear to me very strange, therefore, if the said formation should be interrupted here in Skane on so long a stretch as seems to be supposed, and then reappear again in the same direction on an island in the sea. If I am not mistaken, it was only a short time ago the opinion prevailed that the coal-field of Skane was limited within Luggude hundred. It has been found, subsequently, that this field extends as far down as Eslöf in the hundred of Harjagar, and I cannot see any reason why it should not extend still further south.

True it is there are present sundry silurian and other ancient rock formations, but at no place do they occupy the whole width of the main run, so that they could prevent the coal formation from reaching on past them. But, as before remarked, where coal seams exist in these parts, they are most probably covered by immense layers of alluvial and diluvial deposits as well as with chalk, and are therefore difficult to discover.

For my own part, I consider it to be most probable, whether the coal bed indicated by the blacksmith of Käseberga exists or not, that our Skanian coal formation, which to the north of Helsingborg reaches out into the Cattegat, and to the east of Ystad seems to lose itself in the Baltic, has been created in remote ages past in an immense flood or other inundation of water, which passed over the whole of Skane, south of the southern ridge, with extensions in some places, and, more or less deep, basin-shaped ponds in others.

All at present known of the Skanian coal formation belongs to the lias period.

There exists, however, in England exactly the same kind of lias coal as the Skanian, which we know at present.

In the year 1836, when I was in London for the first time, and one evening returning home to my lodging from the Geological Society, I happened to enter a narrow street where a dealer in minerals had exposed outside his shop, a lot of stones with labels affixed. On looking at them I found on the stones fossil impression of plants, and on the labels some of the names which I had given to the Skanian plant-impressions. I at once purchased the whole collection, and I have it still. On arrival home, I found that all the fossil plants were the same with those which had been discovered at Höor and Höganäs. On the labels is printed the name of *Scarborough*. At Whitby, also, there is said to be a mine of similar coal.

It might have been these which Mr. Bald referred to in speaking of his splint coal in Scotland, for Scarborough is situated on the east coast of England, a good bit to the south of the Scottish border.

The Skanian mine of Stubbarp, near Eslöp, I only know by this, that the schist clay at that place is the richest in fossil plants which I have met with hitherto. It will no doubt be interesting to know, at some future time, if it is equally rich in coal.

CONCERNING RAILWAY AXLES.

By JAMES A. WHITNEY.

From "The Railway Times."

A heavy percentage of railway accidents comes from defective axles, these parts, with tenders especially, being subjected to greater stress than any other portion of the rolling stock. Not infrequently the remote cause of disaster lies in the cheap and, consequently, poor quality of the iron employed; sometimes in a mechanical defect, which, like a sharply turned shoulder at the wheel bearing, seems only a little thing, but is in reality a great one; and sometimes in a change, gradual, and imperfectly understood, which takes place in the molecular constitution of the metal, from the continued

concussion to which it is subjected under heavily laden cars. It is undeniable that proper selection of material, care in manufacture, and thorough testing would largely diminish the number of casualties from this source, but these essentials can only be secured in a high degree at a greater cost than most manufacturers are willing to hazard. Furthermore, the thorough testing, so-called, of axles, as at present carried on, comprises only comparatively crude mechanical means, whereas the object sought can only be fully attained by scientific methods, as yet for the most part only suggested, and even when practically applied, in the majority of cases merely attempted as experiments. The duty of testing the finished axle belongs to the railway company for whose profit the cars are run, and who are directly responsible to the public for the safety of passengers and freight. How railroad managers can be deceived, if careless; or how they can deceive the public, if culpably economical, was shown by the experience, some two years since, of the Mobile and Ohio Railroad Company, on the adoption of an effective, but somewhat expensive, plan of testing the axles furnished to their line. This comprised making each axle in the first place long enough to permit the removal of test pieces from the ends, which were subjected to severe trial, to afford presumptive proof of the condition of the whole. This resulted in the return to the manufacturers of three-fourths of the axles furnished. On the other hand, that good axles can be made is shown with equal definiteness by tests made last year with axles of Bessemer steel, under the direction of the Engineering Department of the Pennsylvania Railway Company. These axles had a diameter of $4\frac{4}{16}$ in., and the standard of trial was that ordinarily adopted by the company, five successive blows from a drop weighing 1,640 lbs. and falling 20 ft. The results showed a resistance in some of the axles to the breaking force $7\frac{7}{10}$ths greater than the common standard, while the weakest of the six experimented upon was $3\frac{1}{2}$ times the standard. The axles were reversed at each stroke, and the test was certainly a severe one. But there is probably a great difference between the effect produced by a small number of heavy blows, and that insured by an almost infinite succession of concussions. An illustration of this general truth concerning the deterioration of metals under jar was shown quite a number of years ago, in experiments with a machine for forging nails, in which the hammers were attached to metallic helves. No matter how strong originally, or how capable of resisting forcible and sudden strain applied for a limited time, the comparatively light, but rapidly succeeding, taps upon the hot nail rod destroyed the integrity alike of steel and fibrous iron, and the machines were finally given up for this reason. On a larger scale, this is what happens when a railway axle is kept in use for a long time, and for it there seems to be no remedy except to condemn every axle, even when made with every guaranty of approximate perfection, after it has run a certain number of miles. But when the constitution of steel and fibrous iron are compared, there seems good reason to believe that the latter when of first quality, and wrought into shape in such wise as to avoid detriment to its natural or inherent strength, will be found practically superior, for the price, to Bessemer metal. But to effect this, some changes in the method of manufacture must be adopted which will be really radical in character, and to many apparently impracticable.

The ideal axle would have its metal as dense as possible, and hence would be shaped mainly by hammering. Its fibres would run unbroken throughout its length, and the tough outer skin which, in wrought, as in cast iron, is much stronger than that within, would be preserved in its integrity, and to secure these conditions, the usual turning off of the surface would in some way be avoided. It has been reported in foreign technical journals that axles have been rolled complete, and when it is remembered that cold rolled shafting is now made that varies but little from the requisite form, the problem of thus perfectly shaping so simple a thing as an axle would certainly appear to be within reach of the inventive and mechanical genius of the time. But the rolling process should be confined to finishing the axle, its shaping being mainly done by the hammer. It is, of course, to be admitted that trials of both rolled and hammered axles have led to directly opposite conclusions as to their value among experienced engineers, but the well-known difference between the compressing action of rolls and the condensing action of hammers points to the superiority of the latter for the heavy work, while to the former should be relegated the finer opera-

tions which should give the symmetry of surface and accuracy of fit ordinarily derived from the contact of the lathe tool. There would further be the advantage in the universal adoption of this mode of fabrication, that it would be difficult to give the sharp corner at the inner end of the wheel bearing, which constitutes one of the most prolific sources of evil in the working of car-axles.

But this practice of making sharp corners, aforesaid, is not only one of the most fruitful of danger, but one of the most foolish that practical men have ever systematically committed in the construction of machinery. It is thirty years or more, since Rankin found and published the discovery, that at such a corner an annular or circumferential groove will sooner or later form, and continue to deepen until the central portion becomes too much diminished to bear the shock of any unusual jar. The ordinary "tapping" will, in aggravated cases, enable such a flaw to be detected, but no skill and no care will guard against the slow but sure approach of danger, because of the unnecessary removal of a few annular chips at the shoulder of the wheel bearing to gratify the whim of the turner.

Allusion has been made to the testing of axles, mechanically, by heavy drops, and although such methods of themselves have a tendency to weaken the axles, just as cannon are weakened by heavy proof charges, there is little doubt that in the present state of the art the practice will have to be retained. The severity of such tests could, however, be diminished, and their utility increased if used in connection with methods of more recent origin and more scientific character. It is some time since Saxby's mode of testing the homogeneity of metal was introduced abroad, and proved capable of indicating flaws undiscovered by ordinary agencies. This was illustrated by boring a hole in a large paddle wheel shaft, plugging it with wrought iron, and then turning down the whole until the place was undistinguishable to the eye; the deflection of the needle, however, showing it in an instant. This plan has been proposed for testing boiler plates and railway rails, and there seems to be no good reason why its application for the same purpose to axles should not give most satisfactory results. The general character of the metal could also, according to the most recent statements of Schott, be accurately determined by the aid of the microscope, the crystallized structure of steel and cast iron being converted in wrought iron into one composed of superposed parallel leaves. Among other notable modes suggested for ascertaining the true condition of axles is that of Sir Joseph Whitworth, which comprises boring a longitudinal hole an inch in diameter centrally through the axle, with the idea of discovering any internal flaw. But this at best would only prove a partial safeguard, and although with Whitworth's perfect machinery the cost would only be one shilling and sixpence, English, it would ordinarily be too great for extended railway practice.

CONTINENTAL COMMUNICATION.

From "Engineering."

The "Times" of the current week publishes three letters of more than usual interest, the first from Mr. John Hawkshaw, the representative of the Channel Tunnel scheme, the second from Mr. John Fowler, the engineer of the proposed Channel Ferry, and the third from Mr. C. W. Eborall, representing the South Eastern Railway. Naturally these communications express opposing interests, though Mr. Eborall and Mr. Hawkshaw are in accord and antagonistic to the Channel Ferry, and while the latter explains in some detail his proposition for improving the present condition of the Channel passage, Mr. Eborall supports the views expressed upon the subject.

There now lie before the public three alternatives for ameliorating a state of things which is universally admitted to call for radical improvement; all three are supported by engineers who stand at the head of the profession, and whose judgment and skill cannot be called in question. The first is Mr. Fowler's steam ferry; the second the tunnel beneath the channel; and the third, also supported by Mr. Hawkshaw, an improvement of the existing means of communication by the establishment of a ser-

vice of vessels of considerable size, to which the existing harbors may be adapted without an excessive cost.

The Channel Ferry, the chief of these three schemes, has just received the sanction of the Committee of the House of Commons, and will shortly be submitted to the consideration of the Lords. It has been before the public for several years, and has been the subject of much discussion in previous committees. Every detail has been carefully considered, and the scheme has received the approval and support of most of the leading engineers of this country, both as regards the harbors and the vessels for which they would be built. The objections urged against the undertaking are chiefly those which have been made by superficial critics, who have lacked either sufficient knowledge or judgment to criticise at all.

The questions raised by Mr. Hawkshaw, however, are of a different class, and, coming from so distinguished an engineer, deserve serious consideration, although, under the circumstances, we should scarcely have expected to have seen them expressed in the correspondence referred to.

Without attempting to form any comparative estimate of the merits of a Channel tunnel and a Channel ferry, Mr. Hawkshaw draws a parallel between the latter and his own subordinate scheme, and maintains that no corresponding result will arise from the expenditure of so large a sum as Mr. Fowler demands for the execution of his work, while for a much smaller sum every accommodation would be obtained.

Mr. Hawkshaw proposes to place upon the service boats of the Holyhead steamer type, 350 ft. long, and drawing 8 ft. or 9 ft. of water, with the necessary alterations to the harbors on each side to receive them. Mr. Fowler's boats would be 400 ft. long, and of course corresponding alterations would be required. Mr. Hawkshaw proposes to run these vessels simply as passenger boats, Mr. Fowler would carry freight trains in the lower part of his ferries, and a certain number of passenger carriages on deck.

So far the difference between these two schemes is one of degree and of detail, not of principle, the only difference worth noting being that in one case trains would be conveyed, and in the other they would not. We think there can be little doubt as to the advantage that would be gained by conveying freight unbroken across the Channel, and herein lies one of the distinctive features of Mr. Fowler's plan, a feature which has received the support of engineers well able to judge as to its practicability. Conveying the passenger trains on deck would also have its advantages, but it would be an experiment easily discontinued if found advisable. We have already pointed out that a general idea exists that these boats are to be constructed of large dimensions, in order that they may have sufficient deck area to carry passenger trains, whereas, in reality, if this portion of the scheme were to be abandoned, the size of boats would not be reduced, because the main object is to obtain steadiness, which would be sacrificed if smaller vessels were substituted. Had Mr. Fowler, after the years of consideration he has given to the matter, arrived at the conclusion that smaller boats would answer the purpose, he would naturally have preferred advocating the less costly, and therefore more easily completed, undertaking. But, as he states in his letter, the saving in time and money between an imperfect and a complete reform is so small that it would be unwise to carry out the cheaper work.

Viewed as a substitute for the present service, and as a make-shift until the tunnel shall be completed, doubtless Mr. Hawkshaw's suggestion would be a wise one; but for many years to come, at all events, the tunnel can be nothing but a scheme upon paper. True, some few thousands have been subscribed for experimental borings and driftways, but we think that no amount of energy or enterprise would encourage the subscription of even £10,000,000 for the construction of a work with such a doubtful engineering and commercial future. Mr. Hawkshaw points out, as an argument for the smaller vessels, and against the ferry, that in winter the Channel boats frequently carry only 50 passengers; but surely such an argument tells with infinitely greater force against a scheme, the preliminary estimate of which is so large, and probably, despite all the sanguine statements that have been shown, none of the promoters of a tunnel imagine that if completed it would pay. At the best it would be a most uninviting means of communication between the two countries, to which even the existing service would be infinitely preferable.

The question then narrows itself to this, which of the two practicable proposals shall be adopted—that of Mr. Fowler, or that of

Mr. Hawkshaw? In dealing with this we may disregard the charges of impracticability that have been brought against the former, and consider only the requirements of the situation, and the measures of usefulness each scheme would offer. We think there can be little doubt upon this point, and we hold that only one unprejudiced conclusion can be arrived at—that in favor of an undertaking by which an unbroken railway communication can be established between this country and the Continent, and which can be completed quickly, and at a comparatively moderate outlay. Half measures would not be found satisfactory in the present, and they would certainly require modification and extension in the immediate future.

CLEAN DRAINS AND IMPROVED MORTARS.*

By MAJ.-GEN. SCOTT, R. E.

From "The Building News."

Although each of the questions involved in the above title is, taken individually, well worthy of discussion at a conference of architects, the connection between them is not readily apparent; but before my paper is concluded, I hope to be able to show you that a relation may be with advantage established between the preparation of hydraulic limes and cements, and that great sanitary difficulty of the age, the purification of sewage and the cleansing of sewers. Whatever may be the diversity of views entertained respecting the ultimate disposal and cleansing of sewage water, all are agreed—agriculturists, chemists, and engineers, whether the advocates of irrigation, filtration, or precipitation—that the sludge must be removed from it. If this be so, the system which effects this with the cheapest materials, which produces the least nuisance in the process, and which yields the most marketable article, has the best chance of success. Now, my plan is to use as the precipitants of the sludge, lime and clay, both of them substances readily procurable everywhere, the one being also of merely nominal value, and the other of little cost as compared with other equally effective precipitants, and to turn the deposit into ordinary articles of trade—viz., cement or hydraulic lime, for which there is in most towns a constant demand; and the scheme is approved by the most eminent authorities in sewage matters. "In all the clarification plans," says Dr. Voëlcker at the Society of Arts, "which have hitherto been put in practice, whether they were the lime, or the sulphate of alumina, or the phosphate sewage process, very little, comparatively speaking, of the precipitating agents have been used, for obvious reasons—reasons of economy. Now, in General Scott's plan a very large proportion of earthy matter, clay and lime, was used, because the object was to bring out a useful product, and it was mainly due to this circumstance that the sewage slush did not enter into putrefaction. He had himself examined at Ealing some of the clay and lime sewage mud which had been kept for nearly three months, and he found it had no disagreeable smell."

Dr. Odling, F. R. S., Professor of Chemistry at the Royal Institution, on the same occasion said that he had seen General Scott's process carried out at Ealing, and he must say he was very much impressed with it." General Scott "recovered the material with which he effected the process in a form more valuable than it was before it was used." "Economically, the scheme seemed to him the most promising of all which had been introduced for the purpose of dealing with sewage."

Dr. Edward Frankland, Professor of Chemistry at the College of Chemistry, and one of the Commissioners for Inquiry into the Pollution of Rivers, gives it in evidence before the Select Committee of the House of Commons on the Birmingham Sewerage Bill, that "he should prefer to deal with the sludge according to General Scott's system—viz., turning it in to cement. . . The process of treatment at Dunton which he would recommend, was to mix lime and clay with the sewage shortly before it reached the tanks. That would produce a complete deposit in the tanks, and would deodorize the sewage, which would flow on to the land without any smell arising from it, and the deposit also would be absolutely inodorous."

After such important testimony concer-

* A paper read at the General Conference of Architects.

ning the economy and efficiency of the process of precipitation by lime and clay, I may venture to take you a step farther, and remind you of the fact that these are the substances which are the essential components of all hydraulic limes and cements. The precipitation which ensues on their addition to sewage water is chiefly due to the combination of the lime with the carbonic and fatty acids which it finds there, but partly also to its affinity for the clay which is added with it. These substances in subsiding carry down with them, first, some of the carbonate of lime which was held in solution in the sewage water, and small quantities of other soluble salts; second, the clayey mineral matters in suspension in it; third, the whole of the organic matter in suspension, together with some portion of that which is in solution.

Now, it is manifest that we have in the deposit thus produced, both as respects the mineral matters removed from the sewage water and in the materials added to produce precipitation, substances analogous to the components of limestones, such as are employed in the manufacture of hydraulic cements and limes. After calcination, the products are perfectly similar, for all the organic matter has been destroyed, the carbonic acid and water has been expelled, and nothing remains but lime and clay, together with some other substances, such as the alkalies, phosphates, etc., which are found in small quantities in all hydraulic limes and cements.

Though it will probably appear unnecessary to most of my hearers to insist upon the inferiority of the pure or fat limes for the purposes of the architect, to those which have hydraulic properties, yet, as it will conduce very much to the appreciation of my process for improving limes that this inferiority should be strongly present to your minds, I will ask you to pardon my quoting also from some distinguished authorities on this point. I wish, as far as possible, to tell my story by the words of those whose opinions must carry weight with you. "If, then," says Vicat, "in what follows, we treat of ordinary mortars, or the mixtures of sand and rich limes, it is because we are compelled to do so to complete the history of the phenomena we have to describe; for it is our most decided opinion that their use ought forever to be prohibited, at least in works of any importance." "We are in the habit of composing our mortars," says M. Treussart, "of fat lime and sand. Our mortars, consequently, have little durability. We shall not obtain durable masonry in the air until we make use therein of hydraulic mortars. In countries where good hydraulic lime is to be had, no other kinds should be used for any purpose whatever."

Sir C. Pasley's testimony is equally strong. "I had ascertained," he writes, more than twelve years ago, "that the pure limes, such as chalk lime, Carrara marble, etc., were utterly unfit for the purposes of hydraulic architecture, as they dissolved away on the outside, and never set at all in the inside of walls exposed to the action of water. But I was of opinion at that time that the mortar of these limes was good for dry situations and for inside work, provided that the external joints were protected against the effects of beating rains, by pointing them either with cement or some superior sort of lime." He then goes on to say that further experience showed him that such pure lime mortar rather dried than set in inside work and dry walls, and he concludes the subject with the pithy remark: "Thus chalk lime mortar, when wet, is a pulp or paste, and when dry it is little better than dust."

We may fairly assume, then, that in all districts in which pure limes are the only limes now available at a moderate price, any scheme which would supply the architect with a good description of hydraulic lime or hydraulic cement without any considerable increase of the cost of his works, would be an important boon. Now, that which constitutes, essentially, the difference between a pure lime, a feebly hydraulic lime, and an eminently hydraulic lime, or a cement, is, as I have stated, and as, indeed, you must all be perfectly aware, the amount of clay present. The results may be modified by the greater or less proportion of iron and alumina or alkalies in the clay, or by the temperature of calcination; but if any one point of difference be selected for purposes of classification, it is the quantity of clay which must be our guide. On this principle limes have been classified by Vicat, as: first, fat limes, when the quantity of clay present in the limestone is less than six per cent.; second, as hydraulic limes of medium quality, when the quantity of clay is from eight to twelve per cent.; thirdly ordinary hydraulic limes, when it reaches fifteen or eighteen per cent.; and fourthly, eminently

hydraulic limes, when it amounts to twenty or twenty-five per cent., the last-named quantity being that which is present also in Portland cement mixture previous to calcination. The resistances which Vicat assigns to these four qualities of limes, commencing with rich or fat limes, are as—

1 ..... 2.3 ..... 3.3 ..... 4 to 6.3

The resistances assigned by Colonel Rancourt de Charleville to the mortars of similar descriptions of lime as the above and quartzose sand are scarcely above 1 ..... 2 to 4 ..... 4 to 6 ..... 6 to 11, and the results obtained by Treussart were very similar to those given by Vicat.

If we endeavor to assign a place to the limes of this country which are to be met with in the market, we shall find that none of them are such as to merit being classed with the "eminently hydraulic limes" of the French writers. It is true that there are certain beds of lias and gray chalk which would yield limes, that would come up to this standard, but they are of very irregular composition, and have never found favor either with builders or architects. Nevertheless, it would be quite possible, by artifical mixtures of lime and clay, to prepare such eminently hydraulic limes of equable composition, just as it is possible, by triturating chalk and clay with water in proper proportions, to produce a mixture which will burn to Portland cement, the strongest cement known. Whether the mixture is made by precipitation from sewage-water, or subsidence from ordinary water, would be of little moment, either in the preparation of lime or cement. We have merely to add the ingredients to the sewage-water at some distance from the outfall into the depositing tanks, and we obtain as perfect an admixture of the ingredients as could be made by the operations of nature in the production of limestones, and probably more constancy in the proportions of the ingredients than nature ever attains. With the object, indeed, of sanitary improvement in our towns, I introduce the lime and clay not only at a distance from the outfall, but at different points of the system of sewerage through the streets. At Ealing the lime and clay are introduced into the main sewer at a point in the village about one mile and a quarter from the precipitating tanks. The lime, previously slaked, and the clay, in the proportion of five measures of slaked lime to one of clay, are mixed together in a tub by means of revolving arms, and the impalpable mixture thus obtained flows into the drains, and passes on with the sewage towards the outfall. The joint precipitating action and clarifying effect of the lime and clay, or other chemicals, is thus promoted to the fullest extent by the natural agitating process to which the mixture is subjected; the sewage undergoes defecation in the drains themselves, and no longer emits the noxious gases which now find their way into our streets and houses; and the precipitate formed exercises a scouring action upon the bottom and sides of the drain, quite obviating the tendency of the sewage to coat them with filthy slimy decomposing matter. The daily flushings which house drains ordinarily receive ought to keep such drains clean, and therefore, under this system, if these be laid with sufficient fall, no lodgment of noxious matters, which will create disease by their decomposition, can take place, unless the street drains are themselves defective. Professor Abel states, in reference to this system, that "the various cement-producing ingredients could scarcely be more thoroughly mixed by the most efficient mechanical appliances than they are found to be in the deposit described, which, therefore, after separation of the water, is at once in a proper condition for conversion into cement by simple calcination." Mr. Hawksley, the President of the Civil Engineers, also advocates this system of introducing the precipitants "in order to allow the chemical matter to mix thoroughly."

The deposit obtained by the lime and clay process is then dried, and subsequently calcined, as is done with the deposit in the manufacture of Portland cement, or the artificial hydraulic limes made at Meudon, near Paris; but it is to be observed that, in the case of the sewage deposit, there is a considerable amount of organic matter which acts as fuel. Indeed, the deposit, when dried, is so combustible as to enable the material to burn of itself, when in quantity and fairly lighted. Accordingly Mr. Bramwell, speaking of the Birmingham sewage, says: "The sludge deposited at Saltley contains about fifteen per cent. of solid matter, and of that forty per cent. is combustible, and could be used as fuel;" and Mr. Hawksley states: "It was practicable to burn the sludge, which contains a great deal of combustible matter, and the experiment of burning the sludge had been

very successfully carried out by General Scott." The destruction of the sludge by fire is indeed a very important point, independently of the manufacture of cement, as it gets rid of every possible germ of disease. It perhaps may, however, occur to you that the process of drying and burning deposit mixed with so much organic matter may in itself be a nuisance; but here again I can produce unexceptional testimony as to the innoxious character of the process. Mr. Bramwell asserts: "No injury would be likely to arise, in the way of nuisance, from dealing with the sludge in the manner proposed;" and Dr. Voelcker says he "wished to bear testimony to the fact that the process of drying and manufacturing the dried material into cement could be carried on in the immediate neighborhood of the town where the sewage was obtained, without creating the slightest nuisance." The kiln at Ealing burns with a down draught, so that all the organic products, which are evolved at a low temperature, and which might create a nuisance, are subsequently raised to a high temperature, and are thus resolved into inorganic matter. The waste products of combustion are carried through flues under a drying floor on which the sludge is prepared for the kiln. The steam, etc., from the drying process may also be carried into the kiln chimney shaft, if necessary.

Experience has taught me that another question suggests itself to most minds in respect of my process, and that is, "Would it not be better to make hydraulic limes and cements in a more direct way than this; and what gain is there, or what advantages have you, in making them through the intervention of sewage?" The advantages in this case, which do not exist in the ordinary processes, are these: First, as I have already stated, the materials used undergo a considerable increase from the lime and clay removed by the precipitants from the sewage water; second, the agitation in a long length of sewer produces better admixture than can be effected by ordinary mechanical appliances; thirdly, the sewage gives up a large amount of fuel, which is sufficient, or nearly so, to effect the calcination of the deposit. But, as Mr. Bramwell very well put it in replying to an objection of this sort, "Supposing the sewage-water contained no ingredient whatever to make Portland cement, but supposing that the materials to cause it to become inoffensive were such materials as made Portland cement, then you put those in, not because you wanted to make Portland cement, but because you wanted to make the sludge harmless and inoffensive; and if you could afterwards make Portland cement, why should you not do so? That he understood to be General Scott's proposition."

I must also meet a third doubt which will be sure to arise in your mind: "Is the cement worth anything when it is made?" I reply again in the words of others. Professor Abel, chemist to the War Department, who has probably examined, in the course of his official duties, more limes and cements than any man alive, stated at the Society of Arts that he had examined the cement made by the process, and "found that it was equal to excellent Portland cement, or in another case to an excellent hydraulic cement of a different character." Dr. Frankland's opinion I have already quoted, and I will add to this testimony that of two distinguished engineers practically well acquainted with the subject. Mr. Hawksley stated before the Select Committee of the House of Commons, already many times referred to: "By adopting General Scott's process, it (the deposit from sewage) could be converted into a very useful and valuable cement." And Mr. Bramwell states, in the same inquiry: "General Scott's plan of converting the sludge into cement is very satisfactory, and from what I have seen of the cement, I have the highest opinion of it."

By omitting the clay an excellent lime for agricultural purposes (because comparatively rich in phosphoric acid, sulphuric acid, and alkalies) may be prepared; but this application has only an interest for the architects as affording an outlet for a portion of the products of the process in cases in which there is a small demand for building materials. As, however, the sewage of a population of 100,000 will only yield from 40 to 50 tons of lime or cement per week, there are few localities in which the demand would not exceed the supply.

I have shown, I hope, in the course of what I have stated, that both hydraulic limes and cements can be produced by the above process. I have also brought evidence that Portland cement of the best quality can be prepared by it; but the manufacture of such cement necessitates

powerful crushing and grinding machinery, and the ingredients must be proportioned with a degree of nicety only to be looked for in large establishments. The so-called selenitic cement, however, is of a "tender" nature, very readily ground, and succeeds perfectly well, though the relative proportions of lime and clay may vary within very wide limits. This material, therefore, with the rough appliances available in places of small populations, is generally the best suited for the objects in view; and for the information of those who know little or nothing of it, I may add that the eminent builders, the Messrs. Lucas Brothers, who have used it very largely, state it to be "so good, and the saving of time in the use of it so great (as it is equal to any cement at the cost of ordinary mortar), we feel sure that it only requires to be known to be appreciated." Selenitic mortar, as is known probably to most of you, owes its remarkable properties to the influence which is exerted upon lime by a small percentage of sulphuric acid intimately mixed in the form of sulphate of lime at the time the mortar is prepared. This treatment enables lime to set as cement by depriving it of its tendency to swell when combining with water, and to detach itself from the surface to which it is applied. In the case of sewage-made hydraulic lime, with twenty-five to thirty-five per cent. of clay present in the calcined mixture, the sewage itself yields a sufficiency of sulphur compounds to produce the sulphuric acid necessary to control the slaking of the lime. The results of the selenitic process on gray chalk lime have been tested by Mr. David Kirkaldy, and his careful experiments show that the resistance of such lime, whether exposed to a crushing or pulling force, is considerably more than doubled by the selenitic action, though the proportion of sand be twice as much as in the lime used in the ordinary manner.

There is every reason to suppose that these results will be surpassed by a hydraulic lime of the same chemical composition as Portland cement, such as can be prepared by the process of which I have endeavored to give you an outline.

A REVIEW OF THE PRESENT CONDITIONS OF NAVAL DESIGN FOR COMMERCE AND FOR WAR.

By C. W. MERRIFIELD, F. R. S.

From "The Annual of the Royal School of Naval Architecture and Marine Engineering."

The day of sailing ships has passed away: the leading traffic, whether of peace or of war, will never again be conducted without the aid of some other prime mover than the winds and tides. There is as yet little indication of any resting-place in the transition, and he would be a bold prophet who should venture to foretell what is to be, in the future, the leading type of vessels, either for commerce or for fight. Our navigation is in a state of transition exceeding that of mere development.

It is occasionally advantageous to pause in our active work, in order to turn round and consider our position, our objects, and our means. I think the present juncture a favorable opportunity for passing these briefly in review, and I propose to dwell especially on those points which illustrate the limits of the power and of the duty of the naval designer.

A ship is a compromise among a vast number of qualities, which, if carried separately to extremes, are incompatible with one another. The design of a ship, therefore, involves a selection from among these, both in kind and in quantity of each, such as on the whole to give the best result for the purpose intended. There is no absolute best, but only the best for the purpose. It is possible to carry out any one quality to an extreme, and it is generally not difficult to give two or three such qualities in a very high degree at some sacrifice of the remainder. Thus cargo-carrying capacity is the leading quality in a barge; while in a life-boat seaworthiness is everything, and in a racing yacht speed. But in all other vessels, and to a certain extent even in these, a specific minimum of the other qualities of a ship must be secured, in order to give it (if I may draw an illustration from animal life) sufficient general vitality to carry out its special function with success. These considerations practically reduce the attainable maximum of any one quality to limits

which, although still wide, are far below the absolute.

The problem of the best ship is therefore an idle one, and the problem of the ship which theoretically fulfils any one quality to its absolute extreme, although conducive to a clear conception of our art, is not of much help in practical design. Our problem is a relative one—to design the best ship for a stated purpose, and subject to stated conditions—conditions and a purpose which must be as clearly understood and specifically stated as the data of any other problem which is presented to us for solution. We cannot, it is true, solve every problem which is presented to us in naval design; our actual knowledge, both of fact and of theory, is deficient in too many respects for that. Let us, therefore, begin by cataloguing what we are able to do.

In the first place, when we know the form of a ship, by means of accurate drawings, we are able to calculate with great exactness its displacement, its capacity, and all those statical properties which depend upon its mere geometrical form. To give mechanical meaning to these, however, it is necessary that we should make ourselves accurately acquainted with the distribution of the weights. Where we have the control of the stowage, as in ships of war, we can do this with a very fair degree of accuracy beforehand, especially if we take the precaution of weighing in and out all that goes over the ship's side. But even if we do not do this, the easy experimental process of inclining the ship by moving a known weight through a known space, will give us very accurately the centre of weight, if we have not calculated it, and will afford us an excellent check upon it, if we have calculated it. We are thus able, by the use of well-established methods, to arrive at all the statical qualities of a ship, if we can have access to her drawings, and can perform a simple experiment just before she starts.

In the design of merchant ships we can dispense with much of this, chiefly for the reason that the designer has practically no control over the stowage, and must, therefore, so design that a considerable variation in this respect shall not be detrimental to the ship; and he must look to the master mariner to see that there is no absurdly bad stowage, and that the ship shall be handled with some regard to the disposition of her freighted cargo.

In sober truth, the primary responsibility for the safety of a ship can never be taken off her commanding officer. Setting aside the dangers of making land, of shoal water, and of collisions, no ship can be made safe in a fool's hands, and it would be quite idle to attempt to design for that. But considering that the ocean has shores and shoals, if safety alone were in question, an exaggerated life-boat would not make a safe ship. Speed and handiness are quite as important qualities for safety as a mere warranty against upsetting.

The dynamical qualities of a ship are more difficult to deal with, and it cannot be said that the present state of our mathematical and physical knowledge enables us to master them completely. Still we are enabled practically to get at most of what we want with a very fair degree of approximation.

We have no theoretical knowledge of the actual form of least resistance to direct motion in a resisting medium, especially in the case of a vessel which is only partially immersed. We do not know this for still water, much less for wave water.* Nevertheless we can calculate, with very respectable accuracy, the probable speed of vessels of all ordinary types, under all usual circumstances. Moreover, the proportions are generally dictated by other requirements than the simple one of the least resistance.

Rolling in wave water depends upon very varied considerations, the most important of which are the following:—

(1.) The statical stability, or stiffness, depending on the horizontal distance between the centres of buoyancy and of weight.

(2.) The distribution of the weights about the centre of weight, depending upon the moment of inertia.

(3.) The form of the ship, including keels, bilges, and bilge-keels.

(4.) The harmony or discord between the proper periodic roll of the ship and the actual periodic time of the waves.

* A mixture of practice and theory has led us with a very fair degree of probability to the conclusion that vessels, moulded upon lines of which Mr Scott Russell's wave system is the extreme limit in the direction of sharpness, come very near the solution of the problem. In my own opinion, his entrance is a little too sharp; otherwise I think he is right in his objections both to a cutwater and bell-bow on the one hand, and to a spur-bow on the other. For long ships, especially, I agree with him in thinking that the floor should be carried well forward, that the bow should rather tumble home than flare, and that the stem should not rake. A projecting bow, such as is in fashion at Hartlepool and Sunderland, appears to me to be not only ugly, but bad in respect of structural strength and nautical qualities.

Stiffness in still water, and steadiness in wave water, instead of usually going together, are discordant and almost contradictory qualities. Stiffness practically means a tendency to keep the deck parallel to the surface; and, in a sea-way, this means parallel to a wave-surface. A ship of much stiffness, rolling unrestrictedly, will usually roll more than the waves, especially a broad, shallow ship. The question of rolling would lead us into far more matter than we have room for here, and it is sufficient to say that very great stiffness in still water is not conducive either to ease or safety in a sea-way. The best preventives (speaking generally) of heavy rolling are deep immersion, moderate stiffness, and well-marked keels, whether false or bilge-keels. While our theory of the subject is far from being complete, or capable of compact expression, we know quite enough to justify us in saying that we can build a vessel, which, if we have the stowing of her, shall very seldom roll uneasily or dangerously.

Pitching is but another kind of rolling; but inasmuch as the longitudinal section is always broad and shallow, a ship can only be prevented from pitching by being long enough to bestride more than one wave at a time. The angular extent of pitching is, however, so much less than that of rolling, that it is only abrupt pitching, like that of short or fine-ended ships, which is disagreeably felt. At the same time, pitching takes up a great deal of the work which ought to be expended in propulsion. The ships that pitch least are long vessels, with the floor carried pretty far forwards.

As regards structural or local strength, there is no difficulty in providing for them both easily and cheaply, in the very longest ships yet constructed; nor will there be any difficulty in designing sufficiently strong for a greater length, even with a small depth or draught, provided a good, strong iron deck—not too much cut up—can be specified for.

Having thus stated, in general terms, the leading matters within the designer's control, I turn to certain points which do not rest wholly with the Naval Architect.

In merchant ships I need only mention two points. The first is stowage. As I have already remarked, the builder can design for a certain margin in stowage; but if the ship is to be run fine in any way, either for sailing or steam performance, or otherwise, that margin must be small. No living man can design any one ship which shall behave equally well with railway bars on her floor, with unpressed cotton piled half-way up her masts like hay on a barge, or with coals or wheat. A ship must be reasonably stowed, according to her designer's intention, if he is to be held responsible for her performance.

The second point is length. Under steam, at moderate speed (seven to ten or eleven knots), long vessels are far more economical, per ton-mile, than short ones; but they require more careful handling both at sea and in making the land. They do not steer so easily as short ships, and it will not do for them to broach to, or to be pooped, in a heavy sea-way.

In all cases the designer has a right to claim the fullest information, not only in general, but also in detail, of the points which his design should fulfil. The absurdity of supposing that he can meet an unknown requirement is sufficiently exposed by being named.

In the merchant service these necessities are sufficiently met by the experience and theoretical knowledge which are already in the possession of the leaders of our profession. In the war navy the requirements are far more complicated and precise, but far less definitely specified to the naval architect. There are, moreover, many traditions of the navy, taken from the practice of sailing ships, which must be laid aside before it is possible to make a proper use of steam power.

I do not propose to enter into any lengthened criticism of our existing fleet. It is necessary to remark, however, that it is sadly deficient in sail power, and by no means strong in steam power. It cannot manœuve under sail, and it can beat only to *leeward*. Over-sparred for steam, under-sparred and over-hulled for sailing, it performs both functions imperfectly, and that is no compensation for not doing one thing well. The ships are also very much over-manned in proportion to their number of guns.

It is idle to talk of good naval design so long as a topman is looked upon as the ideal of a sailor, and cruising as the duty of the fleet. The sailor we now want should be boatman, gunner, and stoker, rather than reefer. A steam fleet should be either at anchor or at half boiler power, except when it is under full steam. Nothing

can be more dangerous than the attempt to manœuvre a fleet in narrow waters and strong tides at low speed. We have it on the highest authority that the manœuvres in the course of which the Agincourt took the ground off Gibraltar were ill-devised and ill-executed. It is worth while to consider whether their chief defect was not the attempt to secure economy of fuel by means of low speed.

Moreover, no ship can be properly designed unless the guns of which it is to be the floating carriage, and the work which it is expected to perform, be settled and specified. When guns were small—six or twelve pounders—it was easy to build a ship fit for either, and then to arm her as thought best, although this did not always prove successful in the old wars. General strength would support small artillery. But you might as well try to mount a thirty-two pounder on a slated house-roof as to put thirty-ton guns in the strongest ship that can be built, without specially designing for those particular pieces. A very small margin of variation is all that can be reasonably provided for.

Underlying all this, however, is the question of the general defence of the Empire. Until a matured scheme for this has been made known to him, the designer must labor in the dark, and work perforce without a settled plan. Are we to sweep the seas, and show front at all points, as we did in the wars of the first French Empire? Or are we to do as we did in the early days of the French Revolution—hold securely some strong and well-selected places, on which we could fall back for supplies, and from which we could issue forth to smite the enemy at every open point? When the naval architect has made himself acquainted with this general plan—a plan which doubtless exists somewhere—when he knows the distances between his coaling, victualling, and repairing stations, and the accommodation to be expected at each—then, and not till then, he can usefully set about the design of a navy. Scattered as our empire is, he must expect to have long distances between each station. Malta, the Cape, India, Sydney, or Melbourne, Vancouver, the Falklands, some West India port, perhaps also the Bermudas, and Quebec or Halifax—these are all the places in which he could have the means of docking and refitting large steam fleets, or in which he could reasonably expect to find heavy guns, spare screw shafts and propellers, or large reserves of sea-stores and coals. In time of peace, or in war with small powers, he may expect to coal at small stations; but in serious war he must expect to find that great reserves, such as fifty or a hundred thousand tons of coal, and the means of docking and refitting, can only be placed at the few points for which strong garrisons can be spared, so that they must be separated by 5,000 or 7,000 miles of sea. If it is intended to blockade a coast, the first step is to seize some island or peninsula, and establish a depot. But this is independent of that general scheme for the defence of the Empire which is necessary to the design of our fleet.

Although advocating, as I have done, the use of full steam power, I do not mean to propose that ships should be altogether without sails. My belief is that steamships should have a fore-and-aft schooner rig, which could be sent down altogether if needed. Increased expenditure of coal will be necessary. I wish to record my opinion that this cannot be avoided if the Navy is to be made efficient. The true sources of economy are to be found in the improvement of the machinery and in the reduction of the crews. The establishments of seagoing ships are now settled mainly with reference to their sails. In full-powered steamships they should be settled with reference to the number of guns and the indicated horse-power. Thirty-five men per heavy gun (of 30 or 35 tons), and, in addition, one man per 50 I. H. P., would, in my opinion, be a sufficient complement in ordinary cases.

An interesting problem to the rising Naval Architect is the arrangement of the steam power auxiliary to the working of the ship. At present donkey-engines for each special work are stowed (without much system) in any convenient corner of the decks. What is needed is, that a careful and complete scheme should be worked out, dealing with the auxiliary steam service as a complete whole, entering into the general design of the ship, in the same way that the rig and the gunnery are now treated.

Ships for coast defence also present a very interesting problem, of which the present solution, in the slow monitor type, is a very defective one. The real data of the problem are as follows: A seagoing vessel of war, in addition to her means of offence and defence, is burdened with the condition of car-

rying coals and stores for a long voyage. From this condition the coast-defence ship is free. The true problem is, therefore, to utilize the reduction of the coals and stores, in providing greater speed or more formidable armament. Floating batteries may be cheap; but unless they have high speed they will be fit for the defence of ports or creeks, not for the protection of coasts.

For the attack or worrying of coasts, the chief thing in which we seem to be deficient is the adoption of a boat-gun of very low range, the effect of which would be to make it unsafe for the land power to establish either reserves or magazines within this long range of any creek which a boat could penetrate. Boat attacks can never be of real power, unless in support of military action; but their incessant worry may either occupy a large hostile force, or, if that be reduced, facilitate military attack and operation. Small draught of water and high speed are the essential conditions of efficient coast warfare.

The task I had set myself in writing this article was to bring into strong relief the following points:—

That the first condition of all successful design is that the requirements and purposes of the structure should be fully understood, both in general outline and in detail.

That for war-ships the general plan of naval defence or attack must be devised, and communicated to the naval designer, before he is set to work.

That the free use of high steam power, and the abandonment of sail power, except as a mere auxiliary to steam, is the immediate future of our Royal Navy.

That the ships' companies must be largely reduced, and that a careful arrangement of auxiliary steam power for the donkey-work must form a part of the design of the ship.

To develop these fully, and to justify them, would exceed the limits of space at my disposal. It is sufficient here to have stated them for the careful consideration of my readers, and especially of my old pupils.

THE STRENGTH OF BUILDING MATERIALS.

From "The Builder."

The paper recently read at the Royal Institute of British Architects, "Our Present Knowledge of Building Materials and How to Improve It," by Captain Seddon, R. E., contains some valuable matter. We print portions of it, and advise those who are interested in the subject to obtain it for themselves from the Institute.*

Engineers have, to a greater extent than architects, been compelled to study the nature and strength of materials, and especially of iron, which (used as it is by the former for every kind of work) has to be dealt with so as to economize both weight and material to the utmost, and to make the best possible use of its enormous powers of resistance to strains of every description. In fact, the extensive use of iron for constructive purposes may almost be said to have given birth to the profession of civil engineering as distinct from that of architecture, the result of which has, I think, been a tendency to too exclusive a cultivation of art on the one side, and of science on the other, to the manifest disadvantage of both professions.

Inasmuch as the architect aims at the beautiful in his constructions, as well as the useful, his profession is of a more elevated character than that of the engineer; for by assisting to cultivate the public taste he leads towards the source of all beauty and purity. Engineers confining themselves too closely to one idea—namely, the theoretical perfection of their work—have fully met the want that gave them birth, have shown how maximum results may be obtained from a minimum expenditure on labor and materials; but in mastering science they have too much neglected art, and even at times justified the absence of any aim at the beautiful, by affecting to rise above such ideas into the regions of the stupendous and the grand; but, unfortunately, in minor undertakings, in little works which cannot aspire so high, we still find the same absence of any attempt to please the more cultivated feelings of our nature; or if the attempt is there, it is mostly too evident that a given sum has been expended upon purchasing a mask to hide not the loveliest of structural details below.

The question which I mean to raise is,

* Sessional Papers, 1871 72, No. 10.

whether we are yet sufficiently acquainted with the properties and strength of the different materials in common use for building purposes to enable us to employ them to the best advantage, or to allow of our calculating with accuracy the amount of material necessary, in every part of a structure, to meet the different stresses called into play?

It may be said by some, What more information do we want than that already within our reach? There are hand-books enough, in all conscience, with copious tables, giving the strength of all kinds of materials under every description of stress, and formulæ for calculating the requisite dimensions of beams, columns, etc., of different forms and under varied conditions; surely we are in possession of all the information any one could possibly require. Nevertheless, I think it must be admitted, on a little reflection, that the present state of our knowledge in these matters is, in face of the boasted enlightenment of the nineteenth century, by no means so satisfactory as at first sight might be imagined; or in any way sufficient to warrant our resting content without making any further researches.

Most of the data upon which calculations have hitherto been based, have been derived from experiments made on picked specimens, too small in size, and too free from such ordinary defects as are sure to occur in larger specimens, to give us very reliable grounds to go upon; the result being that we are forced to supplement our defective knowledge by using large factors of safety; or, in other words, by not straining the material used to anything like its estimated powers of resistance.

Timber.—Taking the subject of Timber first, I cannot perhaps do better than quote from a valuable little treatise lately published by Mr. B. Baker, C. E., "On the Strength of Beams, Columns, and Arches." At page 127 he says:—

"Unfortunately, most of the careful experiments of Tredgold, Barlow, and other early investigators were made on small pieces of timber, straight-grained, and free from knots and other defects; a condition favorable, it is true, to the comparison of the results of mathematical investigation with those derived from direct experiment; but, on the other hand, leading to errors of much greater moment in actual practice, since (as every workman knows) a piece of timber uniformly sound throughout can never be reckoned upon." He then goes on to show the percentage of loss of strength due to the inevitable defects in large scantlings, as follows: A piece of English oak, 2 in. and 1 in. square, gave a result equivalent to a breaking weight of 8½ cwt. applied at the centre of a 1-in. square bar supported on bearings 12 in. apart, giving a calculated stress on the extreme fibres of the bar equal to 7.6 tons, or 17,024 lbs. per sq. in., a surprisingly high, and, as far as practical cases are concerned, a palpably exaggerated result. Whereas, taking a larger scantling of oak, 11 ft. 9 in. long and 8½ in. square, the calculated stress on the extreme fibres, when rupture took place, was only five tons, or 11,200 lbs., instead of 17,000 lbs. per sq. in.; and a larger beam still, 24 ft. 6 in. long, 12¼ in. deep, and 10⅔ in. wide, gave a result equivalent to less than one-third of that given by the small selected piece. He then says: "This reduced amount shows that the average strength of the timber in this large beam was less than one-third of that in the small selected piece; and we think no further illustration is required to show the necessity of neglecting the majority of experiments made on small scantlings of oak, when deducing rules for practical application. We find the same conclusions hold good with reference to Riga, Memel, pitch pine, and other soft woods," the standard bar, 12 in. by 1 in. square, giving a maximum stress on the fibres of 3½ to 4¼ tons per sq. in., whilst experiments on a beam 15 ft. long and 12 in. square give a maximum stress of only 2¼ tons per sq. in.

If we turn to Molesworth's "Hand-Book of Engineering Formulæ," and Hurst's "Architectural Surveyor's Hand-Book," both of which are books purporting to supply all the latest information brought up to date each year, we find the value of the constant to be applied in the formula for beams under transverse stress, given as five for English oak, 5 cwt. being taken as the central load required to fracture a standard bar, 12 in. long and 1 in. square; although the tensile strength of oak in pounds, in Molesworth's Hand-Book, is given as 17,000 lbs., which would give 8½ cwt. instead of 5 cwt. as the central breaking load. Professor Rankine, in his "Rules and Tables," gives 10,000 lbs. to 19,000 lbs. per sq. in., as the tensile strength of oak, and 12,000 lbs. to 14,000 lbs. for fir or pine.

Now let us glance at the crushing strength

of timber, as given by different experimenters.

Rondelet gives the crushing strength of pine as 54 cwt. to 62 cwt. per sq. in., and that of oak as 45 cwt. to 54 cwt.

Tredgold took 36 cwt. for both.

Rennie gives the strength of pine at 14 cwt., and of elm, as low as 11½ cwt. per sq. in.

Hodgkinson gives 92 cwt. for elm, 90 cwt. for oak, and about 54 cwt. for pine.

Lastly, I have here the results of some experiments made by Mr. Kirkaldy, on two logs 20 ft. long about 13 in. square, one of white Riga and the other of Red Dantzic fir, which show, in the first case, a resistance to crushing of 17.5 cwt., and in the last of 15.5 cwt. per sq. in. Both balks failed by crushing, the lateral deflection not exceeding .64 of an inch in either case. These results approximate closer to those made by Rennie than any of the others.

Here is a mass of conflicting evidence, notwithstanding the apparent simplicity of the subject; and yet it is by no means as simple as it seems. The conditions were, no doubt, very different in each set of experiments; the apparatus employed was different, there were different observers, and therefore it is not to be wondered at that the results arrived at differ. In fact, the seasoning alone of the specimens would at once account for a great part of the difference; for green timber, from the moisture in it reducing the lateral adhesion of the fibres, has not more than half the strength of dry timber, and yet if artificially overdried, a considerable loss of strength would be the result.

With regard to the transverse strength of timber beams especially, though the same remarks apply to those of iron or any other material, what would appear to be an important element in their strength, though hitherto omitted from all calculations, is the lateral adhesion of the fibres to each other.

Iron.—I now come to the subject, and a most important one, of iron. Notwithstanding the great advance which has of late been made towards a more perfect acquaintance with the properties of irons of different classes, and notably by means of the numerous experiments made by Mr. Kirkaldy, and the stimulus which has been given to the manufacture of high-class irons, by the rival contests between iron guns and iron shields, it must be admitted, even by those who have made it a subject of special study, that there is very much yet to be learnt about iron; whilst, if we except a small circle, whose special employment has caused them to follow with interest in the track of every experiment which could throw any light upon the nature and properties of the material with which they are chiefly called upon to deal, there is a general lack of knowledge about the whole subject, besides much misconception, which the clear proof of practical experiment will alone be able to sweep away.

As a building material, iron is day by day forcing its way everywhere, and many, who not long ago would have set their faces against its use in structures aiming at a high class of art, no longer hesitate to call in its valuable assistance in order to solve constructive problems which would be beyond the reach of wood, brick, or stone,—at any rate, within any reasonable limits of expenditure. Such being the case, it is essential that its properties should be thoroughly understood by all those who are likely to make use of it for constructive purposes, and that they should not merely order a girder, for instance, to carry a given load, leaving the designing of it to the manufacturer or his agent, whose interest it is to run up the weight, and hence the price, at the expense of quality and good workmanship.

It may safely be said that there is no material so dangerous to trust to, without a full knowledge of its behavior under different conditions, as iron; whilst there is none which varies so much in quality, or in the manufacture of which there is more knowledge, experience, skill, and care required, or which admits of more deception being practised upon the unwary by unscrupulous and dishonest manufacturers.

Now I think that, beyond the difference between cast and wrought iron, and the inferiority of the former, when exposed to the effects of sudden shocks, there is very little accurate knowledge on the subject of the properties and powers of resistance of different classes of iron under varying conditions of stress. Their behavior under different circumstances, such as tension, compression, sheering, bending, torsion, either suddenly or gradually applied, varies so widely according to the description of the iron under trial, that the strongest proof which could be adduced of the necessity for a far wider acquaintance with the subject

is given by the ordinary formulæ in use for calculating the strength of iron girders, etc.

* * * *

STONE.—Passing on to another material, let us see whether we ought to rest satisfied with what we know about building stones.

Here again the majority of experimens made have been upon very small specimens, such as small cubes under compression, whilst the recorded results vary with each set of experiments, according to the amount of accuracy capable of being arrived at by the machinery made use of, as well as the skill and care with which the experiments were made and recorded.

If we take a stone which has been more largely used, perhaps than any other namely, Portland, we learn from Barlow that its crushing strength ranges from about 1,384 lbs. to 4,000 lbs. per sq. in., whilst in the experiments made by this Institute, and recorded in your sessional papers for 1864, the mean resistance to crushing, per sq. in., arrived at was, for 2-in. cubes, 2,576 lbs.; for 4-in. cubes, 4,099 lbs.; and for 6-in. cubes, 4,300 lbs.

According to Rennie its crushing strength may be taken as 3,729 lbs. per sq. in., which has been followed by Molesworth in his "Hand-Book," whilst in Hurst's "Hand-Book" it is given as 2,022 per sq. in.

Now the many varieties of Portland stone, apart from any different method or course pursued in making the experiments, and the amount of seasoning the blocks had undergone, all points which should be carefully recorded, would fully account for the manifest discrepancies between these results; in addition to which, the direction of the natural bed of the stone, which in a small block of Portland might escape detection, would no doubt make a considerable difference. For instance, turning to some experiments by Mr. Kirkaldy on the resistance to thrust of Doulting stone (a Somersetshire oolite),* which I believe to be the only known experiments on this point—if we except two on York paving and Bramley Fall stone, recorded by Rennie, in which the crushing strength both with and against the strata is given as precisely the same, a coincidence too good to be true—the advantage of laying the stones on their natural beds is considerable, increasing rapidly with the increase in height of the block, in proportion to its sectional area; which, I think, is what we should naturally be led to expect, if we look upon the block as approximating more or less, according to the amount of lamination in the stone, to a number of thin columns placed side by side. More experiments, on a larger variety of stones, are much wanted to throw additional light on this subject.

With regard to the supposition that the crushing strength of stone increases with the size of the blocks under trial, there has yet been too little proof put forward on which to lay down any law. In fact, the few experiments made by Mr. Kirkaldy, bearing on this subject, some of the results of which have been placed at my disposal, go to prove that there is no increase in the resistance to crushing, consequent upon increase in the size of the blocks.

With regard to another of the oolites, namely Bath stone, there is, I think, a good deal of misconception, which a careful series of experiments would soon clear up. For instance, Farleigh Down, being a little more expensive than Box Ground stone, is very generally looked upon as the best and strongest description of Bath stone for outdoor use, and is accordingly very often insisted on in specifications, the fact being that, on account of the stone being more difficult to get out of the quarries, especially in large blocks, the price runs a little higher, whilst in strength or endurance it is not known that it can claim any precedence over Box Ground stone. From the experiments already referred to as recorded in your sessional papers, it would appear that Corsham stone is considerably stronger than Box Ground, though this is opposed to the results of other experiments. The durability of Bath stone mainly depends on its being placed on its natural bed, which can only be detected by an experienced eye, or by working the stone; though when not so placed it soon reveals the secret, especially where exposed to the weather, by its cracking and peeling away on the face.

Much also depends on its being well seasoned, or air dried, before being put into the work; therefore the stone should only be got from quarry-owners who keep large stocks of seasoned stone on hand. If quarried in the spring of the year, and stacked at open order during the summer weather, it is doubtful whether Corsham stone is not well able to resist the weather, though it is

* Given recently in our pages.—Ed.

generally considered only fit for in-door work.

Artificial drying, which has sometimes been resorted to, should not be allowed. In one case a large quantity of picked Bath stone, which had been dried by heat, had to be condemned, and, I believe, led to a lawyer's bill, in consequence of the breaking up of the stone under exposure to the weather, owing, I fancy, to the unequal contraction and expansion of the dried and hardened surfaces, and the soft and green interiors of the blocks. I have seen stone, which had worn well exposed to the weather, crumble away on being shifted to the inside of a house.

With regard to sandstones, the information contained in architectural and engineering hand-books is next to nothing; in fact, in Molesworth the whole subject of sandstones is comprised in the information that their crushing strength is 5,000 lbs. to the square inch, which, being an easy round number to remember, might with equal reason be adopted as the crushing strength of all stones.

Very little is known with regard to the transverse strength of different kinds of stone, though there is no doubt that some are much more capable than others of taking a bending stress.

Stone is a material specially unsuited to resist any stress except compression, and it is the true appreciation of the nature of stone as a building material, by the almost exclusive use of it to the best advantage, namely, under compression, by the Mediæval builders, that, to my mind, marks their great superiority, as scientific builders, over their predecessors of more refined classic ages.

In practice, however, we constantly find stone subjected to bending stress, and that further information under this head is required, struck me very forcibly some little time ago on seeing some stone stairs, two stories high, being carefully propped up with wood, many of the steps having split right across close up to the wall. The steps were feather-edged, of Portland stone, $11\frac{1}{2}$ in. treads, and $6\frac{1}{4}$ in. risers, and had been exposed to the ordinary traffic of an office for about 62 years. The treads being much worn, a mason had been at work cutting them down at the top, preparatory to fixing an iron nosing, and filling the treads up level with asphalt, when the step he was at work on cracked close up to the wall, probably from the jarring caused by the strokes of the chisel; shortly after, several of the steps above cracked too, being no longer supported by those below, and being evidently unequal to do the work suddenly thrown upon them. Stairs with the steps only supported in the wall at one end are of constant occurrence, and serious accidents have sometimes occurred from their sudden failure.

Enough has been said, I think, to prove that more knowledge is required as to the special qualities of different kinds of stone, and their applicability to particular uses; but there is still another point about which there is not at present any certain knowledge, namely, to what extent the shape to which stones are cut, and the manner in which they are bedded, affects their strength.

I have the details of one or two interesting experiments to ascertain the effects of lead placed, as is frequently done, between the joints of cut stone columns, etc., with the object of distributing the stress uniformly over the beds of the stone. The experiments were made upon circular blocks of Bath stone (Box Ground and bottom-bed Corsham Downs) 3 ft. long by $10\frac{1}{4}$ in., and 15 in. diameter, or one set twice the area of the other; the lead being cut 2 in. less in diameter than the beds of the stone themselves. The results point to the conclusion that lead so placed between the beds of the stones, reduces the bearing strength of a column to considerably less than that of a column of only half its sectional area, in which the stones are completely bedded. On examining the sheets of lead used in the joints, they seem to have been under compression at a very few points only, and not to have in any way tended to equalize the pressure over the area of the joints. These experiments also seem to indicate that raking out the joints of cut stone work, to save the arrises in cases of any compression of the joints, when bedded in mortar, should not be carried too far. Such questions are, at any rate, worth investigation.

In all experiments upon stone, it is essential to know the exact description of the stone, the quarry it came from, and, if possible, the particular bed in the quarry. The time the specimen has been quarried should be stated, as some stones when green will stand very little stress, but harden considerably, in a longer or shorter time when exposed to the air. If the specific gravity, or weight per cubic foot, of the specimen were given, it would afford

some clue to the state of the specimens experimented on.

While on the subject of stone, I may refer to an artificial stone, widely used in the present day, viz., concrete. I think you will agree with me that a series of carefully-made experiments on the strength of different kinds of concrete would be of great value, under varying conditions, as to the nature of the lime and cement used, the description of ballast, proportion of large and small stuff, and mode of mixing.

With good Portland cement, well burnt and well ground, I should use with confidence for ordinary foundations twelve ballast to one cement, provided I was sure of its being properly mixed; but with ordinary workmen, not properly drilled in mixing the materials, ten to one would probably be more advisable. It would be well to know how much the strength of concrete is effected by the different methods of mixing in vogue. For my own part, I should insist upon the mixing being performed as follows: A yard measure to be half-filled with ballast, then the measure of cement to be added, and the yard measure filled up to the top with ballast. On removing the measure the ingredients get partly mixed, and the cement does not get blown about so much as when placed at the top of the heap; it should then be turned over twice dry, and shovelled into a third heap, each shovelful being sprinkled from the fine rose of a watering-can as it is thrown on the heap, whence it may then be removed to the trenches. This block has been broken with a pick out of a newly-built dock-wall, in which 12 to 1 Portland cement concrete, mixed in the manner described, was employed, and I think it is strong enough for any foundations. In making experiments, the mixing should be done in bulk, at least half a yard cube, being mixed at a time, and not in small quantities, which are more carefully prepared than would be the case in practice; and the blocks should be at least 12-in. cubes.

Passing from concrete to mortars, the results of some experiments made for the Patent Selenitic Mortar Company show that, in mortar made with common stone lime, Burham or grey chalk lime, similar to Dorking lime, was used; 3 sand makes a stronger mortar than only 2 sand, and stronger again than 4 sand; which is probably due to 3 to 1 being about the point at which more sand would weaken the cohesive and adhesive properties of the mortar to a greater extent than its setting or hardening would be promoted by increasing its porosity. With selenitic mortar, 5 sand was the best mixture to resist thrust, then 4, then 6 sand; but, for adhesion and to resist tensile stress, 4, and then 6, and then 5 sand. From which we gather that 3 to 1 is the best proportion of sand to stone lime in common mortar, and 6 to 1 in selenitic mortar, since the latter gives a mortar possessing double the strength of common stone lime mortar. However, in using the selenitic mortar at Chatham lately, 6 to 1 was not found to give such good practical results as 4 and 5 to 1, which is being now used. Although the 6 to 1 mortar set very hard, it was so short that it took longer to work, the loss of time outweighing the saving of sand. The proportions now being used are 4 to 1 for exterior work, and 5 to 1 in the body of the walls. Mr. Street has, I believe, had some further experiments made with the selenitic mortar in connection with the New Law Courts.

Results of Experiments to ascertain the Resistance to Thrusting Stress of Six Cubes of Red Sandstone (bedded with Pine ¼ in. thick).

Test No.	Quarry.	Dimensions. H. L. B.	Base Area.	Cracked slightly.			Crushed, Steelyard dropped.		
				Stress.	Per sq. in.	Per sq. ft.	Stress.	Per sq in.	Per sq. ft.
Ex.		Inches.	Sq ins	Lbs.	Lbs.	Tons.	Lbs.	Lbs.	Tons.
1,892	"O" Ormside.	5.98 5.95 × 5.93	35.28	85,270	2,416	155.4	118,160	3,349	215 3
1,891	"O" do.	5 95 5.95 × 6 00	35.70	84,420	2,356	151 5	117,720	3,298	212.1
1,893	"O" do.	12.00 11.96 × 12.02	143.76	346,620	2,411	155.1	443,930	3,088	198.6
				Mean..	2,394	154.0	Mean..	3,245	208 6
1,895	"D" Dufton.	5.94 6.00 × 5 98	35 88	83,520	2,328	149 7	93,870	2,617	168 3
1,894	"D" do.	5 90 5.94 × 6.00	35 64	81,270	2,280	146.6	92,580	2,599	167 1
1,896	"D" do.	12.00 12.00 × 11.90	142.80	323,880	2,268	145.9	358,230	2,509	161 3
				Mean..	2,292	147.4	Mean..	2,575	165.6

Results of Experiments to ascertain the Resistance to a gradually increased Thrusting Stress of Four Columns of Bath Stone.

"BOX GROUND."

Ex. 577,—

Diameter $\left\{\begin{matrix}10.41\\10.73\end{matrix}\right\}$ = 10.57 in.

Area, 87.70 sq in. Length, 35.06 in.*

61,730 lbs., or 27.56 tons = 704 lbs. per sq. in. Cracked at both ends of column, and very rapidly extended the whole length; part of lead at one end not marked.

1,089 lbs. per sq. in. of lead area.

Ex. 578,—

Diameter $\left\{\begin{matrix}14.97\\15.03\end{matrix}\right\}$ = 15.00 in.

Area, 176.71 sq. in. Length, 34.52 in.*

10,680 lbs. = 570 lbs. per sq. in.: very slightly cracked.

143,950 lbs., or 64.25 tons = 815 lbs. per sq. in. Cracked in all directions; lead at both ends uniformly marked.

1,084 lbs. per sq. in. of lead area.

"BOTTOM BED CORSHAM."

Ex. 579,—

Diameter $\left\{\begin{matrix}14.94\\14.98\end{matrix}\right\}$ = 14.96 in.

Area, 175.68 sq. in. Length, 34.92 in.*

97,920 lbs. or 43.71 tons = 556 lbs. per sq. in. Very slightly cracked.

120,730 lbs. or 53.90 tons = 687 lbs. per sq. in. Only one-half of circumference cracked. Markings on lead prove that the ends had not been formed quite true.

Ex. 580,—

Diameter $\left\{\begin{matrix}10.55\\10.59\end{matrix}\right\}$ = 10.57 in.

Area, 87.70 sq. in. Length, 34.93 in.†

122.280 lbs., or 54.60 tons = 1,394 lbs. per sq. in. Very slightly cracked at one end.

130,810 lbs., or 58.39 tons = 1,499 lbs. per sq. in. One end uniformly cracked all round, the other end but slightly.

The last two experiments reveal the startling and important fact that a column properly bedded is very considerably stronger than one of double the area improperly bedded.

* Bedded with 4 lbs. sheet lead, cut 2 in., less diameter than column according to instructions.

† Bedded with pine ¼ in. thick, cut to same diameter as column, as always recommended by me.

FORCE AND ENERGY—THE CONSERVATION OF ENERGY A FACT, NOT A HERESY OF SCIENCE.

By CHARLES BROOKE.

From "Nature."

In an article entitled "The Heresies of Science," published in a recent number of the "London Quarterly Review," two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution, and the Conservation of Energy.

On the doctrine of *Evolution* the writer has nothing to say.

Before discussing with the "Reviewer" the validity of the *Conservation of Energy*, it is quite necessary to define the terms which may be employed, such as Force, Energy, Potential, Sound, Light, Heat.

It is much to be regretted that a far greater degree of logical accuracy in the use of terms than is usually met with, does not exist amongst even the ablest writers on physics, for many of the arguments adduced against physical principles lie not against the principles themselves, but against the indefinite language in which they have from time to time been expressed. There is probably no term employed in physics that has been more misapplied, and in its misuse has led to greater confusion of ideas, than "force."

Force has been thus defined by our ablest modern physicist:‡ "What I mean by the word force is the source or sources of all possible actions of the particles or materials of the universe."

This definition of force is substantially the same as the writer's definition§ to which the reviewer takes exception, but which may perhaps with advantage be thus amplified: Force is a mutual action between the atoms or molecules of matter, by which they are either attracted towards, or repelled from, each other; and by this action energy is imparted to the matter put in motion. It may be further remarked that force is essentially either attractive or repulsive.

The writer sees no reason to amend his definition of "Energy"—namely, that it is *the power of doing work*. It may, however, be remarked that the existence of energy in matter implies the existence of motion, and *vice versâ*; but it by no means follows that

‡ Faraday, MSS. Croonian Lectures on Matter and Force, by H. Bence Jones, M.D., p. 35.

§ Introductory chapter to the Sixth Edition of "The Elements of Natural Philosophy."

motion and energy are convertible terms, for motion means only the act of moving, or changing the position occupied in space.

The term "Potential" applied to force or energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it. In the same manner the repulsive force of the molecules of the highly ignited gases into which gunpowder is resolved by ignition may with equal propriety be said to be potential in the unignited powder.

The remarks with which the writer's interpretation of the terms "force" and "energy" have been met by the reviewer may here be appropriately noticed. Quoting the introductory chapter already referred to, he adds: "His doctrine regarding the nature of force has thus no connection with sound philosophy; by force Mr. Brooke evidently means what other advocates of conservation mean by potential energy." Does then sound philosophy consist in the impossible task of agreeing as to the meaning of terms with those who do not agree amongst themselves? Or is sound philosophy monopolized by, and crystallized in, the opinions of the reviewer? Sound philosophy would seem to consist rather in assigning appropriate meanings, involving no inconsistency or contradiction, to terms of frequent occurence in all works on physics.

If the above definitions of force and energy be accepted, it is obvious that the term "force," as used by Grove, Tyndall, and many others, means sometimes force and sometimes energy. Thus, for example, "the conservation of force" becomes a simple truism, for, its exercise being a function of matter, force must necessarily be coeval with matter. The reviewer (p. 22) thus quaintly expresses the relations of force, energy, and motion: "A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." Motion, it is presumed, can mean nothing else than the act of moving; but how the act of moving, whichever way we look at or think of it, can either produce energy or do work, it is difficult to conceive; has this "any connection with sound philosophy"? and does not the reviewer here himself "forget that each thing is itself, and not something else?" (p. 22, l. 89).

The writer would commend to the serious attention of his brother-physicists the above definitions; he would also submit the following definitions of sound, light, and heat, the former of which has, however, been elsewhere declared incapable of definition,* as well as by the reviewer (p. 22). Sound may be defined to be the impression produced by certain vibratory movements of matter upon appropriate sensuous organs, including both the receptive and perceptive apparatus. Whether the tympanum be incapable of receiving sonorous vibrations, or whether it vibrate sympathetically while the structures of the internal ear are incapable of appreciating its vibrations, there can be no sound.

And why may not the same definition apply to light and heat? It is, in fact, far from improbable that the perceptions of light and heat may result from the impressions produced by the same identical vibrations on different perceptive organs; that of light on the eye and its nervous appendages, that of heat on the organs of common sensation. In common parlance, the terms sound, light, heat, will doubtless continue to be applied indiscriminately to the vibratory motion producing, and to the impressions produced; and to this there can be no objection, provided no advantage be taken of the misnomer to found thereon an assumption of the identity of the proximate cause and the resulting effect.

The reviewer has sought to kill two birds with one stone, and has made a vigorous onslaught against the conservation of energy in general, and the writer in particular, regarding the theory of latent heat; but it may reasonably be questioned how far "sound philosophy" is shown in attempting to convict an author of admitting an insuperable difficulty in the adoption of a given principle by quoting his statement of the difficulty, and coolly suppressing his immediately subsequent explanation of it. He thus quotes the writer†:—"Latent heat has ever been held up as the great stumbling-block of the dynamic theory, because it is impossible to conceive motion to be reduced to a state of quiescence, but remaining still ready to start again into action."

* "A logical definition of sound is impossible." Dr. M'Cann on "Force and its Manifestations," a paper lately read before the Victoria Institute.

† Elements of Natural Philosophy (p. 786).

But instead of continuing the quotation thus: "This, however, is merely a confusion of ideas, the fact being that when any substance passes from the solid to the liquid, or from the liquid to the gaseous form, a certain portion of the impressed heat-force is continuously occupied in overcoming molecular attraction, and thereby effecting the change of form; and this heat cannot be imparted to other bodies so long as it is occupied in maintaining that change," he ventures to state: "In this we quite agree, and hence we think it unnecessary to give Mr. Brooke's reasons for believing a doctrine which he allows to be inconceivable"!!! Whether "sound philosophy" or not, is this common literary honesty? What Mr Brooke allows to be inconceivable is obviously not the doctrine itself, but the conclusion drawn from fallacious arguments adduced in opposition to it; for to assign reasons for believing what one allows to be inconceivable would be nothing less than pure and simple evidence of mental alienation.

The fact is that the term "latent heat" is an unfortunate one, and has mystified the reviewer as well as many others. It ought long ago to have been consigned to the limbo of discarded hypotheses, together with the material or corpuscular theory of heat from which it arose. If heat consisted of material particles, it might be supposed to become latent among the molecules of grosser matter, just as a handful of shot, if dropped into a box full of marbles, would lie hid amongst them, and be lost to sense, and would so remain until shaken out again; but mere vibratory motion cannot be theoretically dealt with in this fashion.

A much better term would be *engaged* or *occupied* heat, for the so-called latent heat is wholly engaged or occupied in maintaining the change—first from the solid to the fluid state, and secondly from the fluid to the gaseous. The facts are very plain: a pound of water at the temperature of 0 deg. C., or the freezing point, mixed with a pound of water at 79 deg., yields two pounds at the mean temperature of 39.5 deg.: but a pound of ice or dry snow at the temperature of 0 deg., mixed with a pound of water at 79 deg., yields two pounds of water at 0 deg., because the 79 deg. of sensible heat in the water are now employed or occupied in maintaining such an amount of vibratory motion in the molecules of the ice, that they are no longer able to obey that polar attraction by which they were previously aggregated together in given directions so as to form crystals (for though not so evident in ice, the crystalline character of snow is notorious), and the heat-energy, being thus occupied in doing work, is incapable of doing any other work, as for example on the organs of sensation, at the same time; on the principle that you cannot "eat your cake, and have it too." The same reasoning applies to the change from the fluid to the gaseous state; but in this case a much larger amount of thermic energy is employed in so far removing the molecules from the sphere of each other's attractions that the balance of their mutual forces is repulsive, and so long as that repulsion is maintained, the dry steam manifests all the properties common to the fixed gases. "Latent" heat, then, when properly understood, ceases to be a "stumbling-block to the dynamic theory of heat."

One finds one's self occasionally brought by circumstances into an unwelcome generalization. Thus the reviewer, speaking of the supporters of "conservation" in the lump, says (p. 21), "they take it for granted that force is motion and nothing but motion." This the writer entirely and absolutely denies. The reviewer then, immediately preceding his observations on the writer's views, quotes from Prof. Bain that "Inert matter in motion is force under every manifestation." This is plainly an abuse of language, in which the writer, as one of the "they," declines to participate; inertness and force are hardly concomitant ideas, and matter, whether in motion or at rest, is assuredly not force. The term heat-potential adopted by Mr. Rankine is admissible only in relation to heat as previously defined; the thermic energy can hardly be termed potential while it is employed in doing work.

The reviewer (p. 19) quotes, and objects to, the explanation of latent heat offered by Prof. Tait, that while sensible heat is *motion*, latent heat is *position*. The writer must acknowledge his inability to derive any definite idea from this statement of Prof. Tait; he cannot therefore express either assent or dissent.

The writer must plead guilty to having made use in the treatise above referred to, in common with many others, of a phrase which is not strictly accurate, viz., that sound, light, heat, and electricity are modes of motion. It would be more exact to state

that they are so many forms of energy, or effects due to matter affected by certain modes of motion.

It is rather surprising that the reviewer should have ventured to dogmatize on the very slender knowledge either of physical facts or hypotheses that he evidently possesses. Thus he states (p. 33, note) regarding the investigations of Dr. Joule :—

"By means of machinery a weight of 772 lbs. is made to turn a small paddle-wheel placed in one pound of water. Dr. Joule found that the descent of the weight with a given velocity through 1 ft. raised the temperature of the water exactly 1 deg. F." Now this small sentence contains a curious concatenation of errors. Dr. Joule never employed a weight of 772 lbs., for the weights he employed were all under 30 lbs.; he never employed the energy acquired by his descending weights in stirring an exact pound of water, for the stirring vessel was not constructed to hold that particular quantity; neither did he find that the descent of the weight with a given velocity raised the water exactly 1 deg. F., for the velocity of descent has nothing whatever to do with the result; since precisely the same amount of energy is acquired by a body in descending through the space of 1 ft. by the action of gravitation, whether one minute or one month were occupied in the descent. For the sake of convenience, and in order to avoid a source of error, it was desirable only that the weights should descend slowly, and with a tolerably uniform velocity.

So much for the reviewer's knowledge of facts; now as to his knowledge of theories. He writes (p. 31):—

"The supporters of the doctrine of the indestructibility (? conservation) of energy have adopted a method the reverse of scientific. They start with the assumption of perpetual motion means of transformation. In order make facts fit their hypothesis, they take for granted that heat, light, electricity, and magnetism are modes of motion, but not requiring a material basis to account for their phenomena. Some, however, seem to be aware that motion of necessity implies something moving, and that this something must be matter in some of its forms, and that consequently it is a great mistake to suppose that the dynamical theory is inconsistent with the materiality of heat. Finding that they have been a little too hasty in getting rid of the old imponderables, they are now quietly bringing them back under a new name, hoping, doubtless, that their few remaining friends may not be able to recognize them. Instead of the 'imponderables' we now have the 'luminiferous ether' which fills stellar space, and even permeates all the grosser forms of material existence."

This sentence expresses a gross misrepresentation of the course of philosophic thought. Can the reviewer point out a single physicist who for a moment doubts that "motion of necessity implies something moving, and that this something must be matter in some of its forms?" It is an axiom that not "some" but all must obviously admit. When it was supposed that light and heat consisted of material particles projected with immense velocity from their radiant sources, and that electricity and magnetism were "fluids" travelling with similar velocity, it might be assumed that inter-stellar space is an absolute vacuum; but when the progress of physical knowledge developed phenomena which were partially or wholly incapable of explanation on this hypothesis (such as diffraction and interference, and subsequently those of the polarization of light and heat), but which became perfectly intelligible on the hypothesis that these forms of energy consisted in vibratory molecular motion transmitted with the same great velocity, the existence of a highly attenuated and elastic medium as the denizen of infinite space, became a necessary part of the theory; and this, in unavoidable ignorance of its precise nature, was termed "ether." Hence, in direct opposition to the reviewer's statement, physicists take for granted that "light, heat, electricity, and magnetism" do require a "material basis" (that is, matter as a means of their transmission) "to account for their phenomena;" and his dictum about physicists getting rid of the old imponderables, and now bringing them back again, is unmitigated nonsense.

The attribute of imponderability has been ascribed to the hypothetical substance "ether" by many physicists who hold that it monopolizes the property of transmitting the waves of light and heat, and is therefore intersticially deposited in all kinds of matter. The writer is, however, more inclined to believe, with Mr. Justice Grove, that all kinds of matter are susceptible of these vibratory motions, and hence that the hypothesis of *intersticial* ether is gratuitous; his reasons for such opinion, being

elsewhere in print, need not be here repeated.*

It may be further suggested that, for all that is positively known to the contrary, all kinds of matter may possibly be susceptible of a fourth state or condition, which may be termed the "ethereal," and which in tenuity and elasticity may be as far beyond the gaseous as the gaseous is beyond the fluid state; possessing also the mechanical properties of a jelly, rather than those of a gas.

The writer can hardly be expected to take up the cudgels for others against the reviewer; but in concluding the remarks on this point, it may be observed regarding the reviewer's emphatic denunciation of an alleged discrepancy between Grove and Tyndall, that the discrepancy exists only in his own misunderstanding the quotation from the "Correlation of Physical Forces," viz., that "it requires no great stretch of imagination to conceive light and electricity as motions, and not as things moving," in which the writer clearly contrasts the undulatory and corpuscular theories, as commonly understood. Enough has now been advanced to show that the reviewer need not look very far from home for a conspicuous example of that which he has so freely attributed to the unhappy physicists, namely, that they do but "darken counsel by words without knowledge" (p. 23).

The equivalence of dynamic and thermic energy is the only one that has as yet been determined quantitatively. He must be a bold man who denies that the sun shines at noonday; and scarcely less audacious is the assertion of the reviewer that the experiments of Dr. Joule do not confirm this equivalence. Dr. Joule conducted four distinct series of experiments,—three series on the amount of thermic energy produced by molecular friction in stirring respectively water, oil, and mercury; the fourth, on that produced by the friction of two iron surfaces against each other. The four numerical results accorded very nearly, and after assigning to each result its weight, according to its estimated liability to error, he deduced the mean value of 772 foot-pounds as the dynamic equivalent of thermic energy.† In the metrical system, in which the units of quantity are one kilogramme, one metre, and one degree in the centigrade scale, the above equivalent is represented by 424 dynamic units, which, for brevity's sake, we may as well agree with the French in calling "dynams."

The reviewer, in ignorance probably of the amount of labor bestowed on this subject, seems to imagine that by ignoring Dr. Joule's results, he has demolished the basis of thermodynamics; but if so, he is grievously mistaken. It is a remarkable and unprecedented confirmation of this theory, that the numerical results arrived at by three distinct methods of investigation, in the hands of as many independent physicists, should be found to agree within very narrow limits of error.

It has been found by experiment that a less amount of heat is required to raise a gas maintained at a constant *volume* one degree of temperature, than when the gas is allowed to expand under a constant *pressure*. Suppose, for example, that the gas be enclosed in a vertical cylinder under a piston of 100 sq. in. area, the atmospheric pressure on this piston will be 1,500 lbs., and the raising this piston is equivalent to raising a weight of that amount. Dr. J. R. Mayer, assuming that the difference in the quantities of heat in the two cases above mentioned is equivalent to the work done by the expanding gas, proceeds to determine the numerical value of these equivalent quantities. Taking the specific heat of air to be 0.267, as at that time determined by the observations of De la Roche and Berard, he found the dynamic equivalent of an unit of thermic energy to be 367 dynams. But if, in the calculation of this number, the more careful and accurate subsequent determination of the specific heat of air by Regnault be substituted, namely, 0.2375, the result gives as the equivalent 426 dynams; a result almost identical with that of Dr. Joule, but based on purely theoretical considerations.

Subsequently M. G. A. Hirn* pursued a course of observations exactly the reverse of that of Dr. Joule,—namely, to determine the amount of heat converted into work in the steam-engine. Taking it as an axiom, which he showed to be in strict accordance

* Lecture on Force and Energy, delivered at the Royal Institution, "Medical Times and Gazette," July 8, 1871.

† For the sake of those readers who are not already familiar with this subject, we may state that a foot-pound is the amount of energy acquired by a weight of 1 lb. in descending through the vertical space of 1 ft., or, in other words, the amount necessary to raise 1 lb. 1 ft.; and the numerical equivalent here given means that 772 dynamic units are equivalent to the amount of thermic energy required to raise the temperature of 1 lb. of pure water, at or about the mean temperature of the air, 1 deg. of Fahrenheit's scale.

* Cosmos, v. 22, 1863.

with analytical investigations, that the difference between the heat existing in the steam as it enters the cylinder and that remaining in it after its exit, must be the thermic equivalent of the work done in and by the engine (which difference in the best constructed engines amounts to about 5 per cent. of the total heat due to the combustion of the fuel), he determined the value of one thermic unit to be 425 dynams; a remarkable result, and intermediate between those previously obtained by Mayer and Joule. In the face of such overwhelming concurrent evidence will even the reviewer be still bold enough to assert that the conservation of energy is a myth?

The principle of the dissipation of energy, as a corollary to that of its conservation, is of course equally ignored by the reviewer; but as his remarks on that point have no relation to anything beyond his own inner consciousness, it must here suffice to give a familiar illustration both of the conservation and the dissipation of energy in the action of the rifle-ball. This reaches the target with less velocity, and consequently with less energy, than it possessed on leaving the muzzle; a portion of its energy has been expended in producing heat by friction against the particles of air between which it passes, which is dispersed through the surrounding atmosphere, and thus becomes *dissipated*. On reaching the target the progressive motion of the mass is arrested, and converted into molecular motion, which is cognizable only as heat, by which the mass is reduced to the fluid state, and splashes of molten metal are scattered in all directions. These again impart their heat partly to the air through which they pass, partly by radiation into space, and partly to the ground on which they fall; and thus the whole energy of the ball becomes dissipated. An analogous explanation will apply to all other cases of the dissipation of energy; thus the principle may be indefinitely illustrated, but it is hoped that it has now been sufficiently established, that the conservation of energy is a fact, not a "heresy of science," the reviewer's opinions to the contrary notwithstanding, for

> A man convinced against his will
> Is of the same opinion still.

Want of space forbids the consideration of the larger cosmical relations of the conservation of energy to both organic and inorganic existence; one proposition only shall be alluded to as having been by some writers rather overstrained, viz., that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity. This may be taken as a postulate, and is probably true; but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated. For example, two stones of equal weight lie, one at the bottom of a well, the other on the ground at its edge; both are raised and placed side by side on the top of the windlass frame; much more work has been done on one than on the other; but can any one predicate in which of the two the greater amount of potential energy resides? Or is there any conceivable difference in the amount of energy acquired by either, otherwise than as dependent on its descending through the greater or the lesser distance?

It may, in conclusion, be remarked with much regret that the principle of the conservation of energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence. To the mind of the writer, and, it is earnestly hoped, to that of most of his readers, the indisputable establishment of this principle conveys only a more exalted idea of that infinite wisdom by which the perpetually recurring transformations and interchanges, not only of the materials, but also of the powers, of Nature are rendered subservient to predetermined laws, which govern the comfort and welfare of all created beings.

THE FALLING SPRINGS RAILWAY.—The contractor for the work of grading and preparing the roadway for the East St. Louis, Cahokia and Falling Springs railroad, is pushing forward that work with great rapidity. This road is intended to serve the double purpose of a levee to Cahokia and furnish an excellent railroad bed to Falling Springs, a fine rural locality back of Cahokia, where there is a beautiful fountain of clear water pouring out from beneath a cliff of rocks, and will open to use some of the finest stone quarries in St. Clair county.

THE zinc mines of New Jersey yielded about 20,000 tons of ore last year.

THE NARROW GAUGE.

At the recent convention at St. Louis, held in the interests of the Narrow-Gauge system of railways, the following report, which we copy from the columns of the "Chicago Railway Review," was submitted by a Committee appointed for the purpose, early in the year:

MR. PRESIDENT,—Your Committee to whom was delegated the duty of preparing a Report upon the "peculiar merits and advantages of the narrow-gauge railway system" beg leave to recommend the adoption of the following resolution:

Resolved, That having found the 3 ft. gauge so numerously represented in this Convention, *they recommend that it be adopted as the Standard Narrow Gauge by all roads where there are no particular reasons for adopting a less gauge.*

Because (1) a uniform gauge is very essential to the final success of the system; and, (2.) Because they believe, if it is adopted on all narrow-gauge roads, that it will not be many years before the 3 ft. standard roads will have through connections North, East, South, and West, with a better system of interchange of cars and a less amount of transfer than there is now on the 4 ft. 8½ in. of the North, the 5 ft. of the South, and other gauges now in operation.

They also recommend the adoption of 24 inches as the standard height for the centre of draw-head above the rail.

In the call for the Convention, twelve points are suggested for discussion, in order to elicit the fullest information in regard to both the construction and the practical operations of the narrow gauge. Your Committee have been unable to devote as much time to the consideration of the subject as its importance demands, but have touched upon them all, and in doing so have been careful to understate rather than overstate the capacity and numerous advantages of the new system.

I.—THE WANT OF RAILWAY FACILITIES

is acknowledged by all classes, and the valid reason why any locality is without the same, is simply a financial question. All sections of the country would be in possession of railway transportation, if they possessed the means, or could borrow the funds at a reasonable rate of interest; and your Committee believe that any system which tends surely to that end, namely,—the sale of bonds at or as near par as possible, and at the standard rate of interest, is and will be of vast benefit to the public; and as the narrow gauge will do all the business of any section of the country, with a much less bonded debt, it tends to give a better security to the bonds and stock of the roads, making a better sale for the same, and in that way furnishing many feeders to our present through lines, which would not otherwise be constructed, and soon connecting local lines so as to make new through lines of the 3 ft. gauge, North and South as well as East and West.

II.—COMPARATIVE COST OF THE OLD AND NEW SYSTEMS.

Taking what are called first-class roads, equipped to do the business of the various localities through which they run, your Committee feel justified in the following conclusions: (1.) That in very rough, mountainous countries where the transportation of heavy ores, such as gold, silver, copper and other minerals in bulk, before reduced, so as to collect the same at the various smelting works, with the coal, wood and fluxes used in the reduction and manufacture of the same, and where it is not necessary to run fast or time trains,—the cost of construction of a 3 ft. gauge road will not be over one-fifth of such roads as the Erie, Penn. Central, and Baltimore & Ohio; and that the capacity of the cheap road can at any time be increased by capital so as to do all the business, for all time to come; thereby saving a large amount in first cost and interest on the same, which is the strongest possible recommendation for capital to invest in narrow-gauge cheap roads, rather than in the expensive broad-gauge. (2.) That in the broken, rolling country, where most of our roads are constructed, the saving will be about as 1 to 2: namely, that the narrow gauge will cost about one-half as much as the present broad gauges have cost. Your Committee would respectfully ask, and earnestly urge all parties interested in the construction of railways, that this part of their report be thoroughly investigated; and that the question of what the present broad gauges have cost, be examined as carefully by narrow-gauge advocates, as has the question of what their

roads do, and will cost, by the friends of the broad gauge—not forgetting to look into what broad-gauge advocates say they can do in the way of reduction of dead weight, but which none of them can afford to attempt, on account of the vast amount of capital involved. (3.) That in the slightly undulating prairie or plains country the cost of construction of a first-class narrow-gauge passenger road, with the equipment suitable for a large freight as well as passenger business, will not exceed three-fifths of what a broad gauge would cost with what is now called first-class equipment and road-bed; and that the real comforts and safety of the narrow gauge are fully equal to those on the three great Government roads: the Union Pacific, Kansas Pacific, and the Central Pacific.

III.—OUR MEANS OF CONSTRUCTING THE BROAD GAUGE, AS COMPARED WITH THE NARROW GAUGE.

Railway officials are so well advised in this question, that it is only necessary to say, that the obtaining of a sufficient amount of money to construct a road on a cash basis is the only real difficulty to be overcome. It is a self-evident fact that it is very much easier to obtain $10,000 per mile than it is $30,000. It cannot be denied that we have numerous localities, North, East, South, and West, in which railway facilities are imperatively demanded, nor will it be denied that in many instances it would be impossible to obtain the means to construct them broad-gauge; or if able to do so, that when done they would not command sufficient business to sustain them. Their excessive cost, entirely disproportioned to their light business, would necessarily compel them to charge high rates in order to pay operating expenses, and interest account, and thus retard, rather than stimulate development. Your Committee are unable to see the propriety of thus unnecessarily taxing the industries of the country, especially when the required facilities may be obtained by the adoption of the narrow gauge at one-half the cost, thereby placing it within the means of the localities referred to, not only to provide themselves with railways, but with low rates. Admitting that $10,000 per mile will accomplish the desired object, your Committee are unable to see the economy of investing $20,000 per mile and thereby incurring the entailment of high rates for all time.

As to narrow-gauge supplies, such as rail, locomotives, cars, etc., the construction of the Denver & Rio Grande, as well as many other roads from 20 to 150 miles in length, now fast being completed, and even more rapidly than most broad-gauge roads have been constructed, shows most conclusively that American skill, ingenuity and energy has been equal to the emergency. In conclusion, upon this point we beg leave to remark that we are clearly of the opinion that an impartial and thorough examination of the subject, cannot fail to convince the most skeptical of the great advantage in favor of the narrow gauge, in first cost, and the consequent money-saving results to be obtained thereby in the more rapid development of our resources.

IV.—THE COMPARATIVE COST OF OPERATING THE TWO GAUGES.

Your Committee cannot find that it has ever been claimed by the broad-gauge advocates that their system can be operated for less than can the narrow in any case; while the opinion and practice of all roads prove when the business exceeds a certain extent that it is cheaper to construct a double track, the interest on the same to be added to the operating expenses. It should be borne in mind that the double track narrow gauge will not cost as much as the broad gauge single track; hence the narrow can under all circumstances furnish cheaper transportation than the broad. In seeking to develop our resources we must not lose sight of the fact that low rates of transportation will accomplish the desired end much more rapidly than high rates, which frequently prevent, and in all cases retard development. In examining the question of

Passenger Cars.

Gauge.	Weight of car in pounds.	No. Passengers full load.	Lbs. dead weight per passenger.
Broad	38,000	56	678
Narrow.	12,000	36	333
	26,000	20	345

Pounds dead weight per passenger carried on roads in Massachusetts and New York, in 1870.

Massachusetts .... 1,250 | New York 2,784

the comparative cost of operating the two systems, we will first show the difference in the weight of the rolling stock and machinery, and then trace the effects produced in each case to a final result.

The preceding table shows the proportions of paying and dead weight in the cars of the two gauges when loaded to their full capacity.

In this case, the narrow-gauge coach, weight 12,000 lbs., carries when full 36 passengers, with a dead weight of 12,000 divided by 36 = 333 lbs. per passenger, while the broad-gauge coach, capacity 56 passengers, weighs an average of 19 tons, giving a dead weight of 38,000 divided by 56 = 678 lbs., a difference of 345 lbs. per head in favor of the narrow gauge. But these coaches seldom run full, in which case the advantage will be still greater in favor of the narrow gauge. To test the comparison still farther, we will suppose that we have 38 passengers, two more than the small coach will accommodate, making it necessary to put on a second one. In this case we will have two narrow-gauge coaches weighing 24,000 lbs., or 24,000 divided by 38=634 lbs. per passenger; while by the broad gauge we have 38,000 divided by 38=1,000 lbs. dead weight per passenger, or a difference of 366 lbs. per head in favor of the narrow gauge.

But let us apply still another test. We will suppose that we have two narrow-gauge car loads, 72 passengers, or 16 more than can be accommodated with one broad-gauge coach, necessitating the use of a second one. The account will then stand as follows: Two narrow-gauge coaches, 72 passengers, 24,000 divided by 72=333 lbs. per passenger; while by the broad gauge it will be, two coaches 76,000 divided by 72=1,055 lbs. per passenger, a difference of 722 lbs. per passenger, or a total of 52,000 lbs., or over 26 tons saving in dead weight in favor of the narrow gauge in only two cars. The dead weight per passenger on the New York roads for 1870, was 2,748 lbs., exclusive of baggage, with an average of 13 passengers per car. On a large majority of roads the average dead weight is much greater. The passenger coaches, then, on the New York roads, run about one-fourth full. For the purpose of a farther comparison, we will assume that our broad-gauge railways average the same number (13) per car. On this basis, the table of paying loads and dead weights is arranged thus:

Kind of traffic.	Gauge.	No. of passengers per car.	Weight of car in pounds.	Total paying load in lbs.	Dead weight per passenger in lbs.	Gross Load in lbs.
Passenger.....	Broad	13	38,000	1,950	2,923	39,950
	Narrow.....	13	12,000	1,950	923	13,950
			26,000	....	2,000	36,000

A difference of 26,000 lbs., or 13 tons, in favor of the narrow gauge, or 2,000 lbs. per head for each passenger. Assuming the weight of the broad-gauge car to be only 15 tons, or 30,000 lbs., the difference in favor of the narrow-gauge car will still be 18,000 lbs., or 1,384 lbs. per head for each passenger, as against 923 lbs. per head by the narrow gauge. Of course the travelling public pays for this excess of unnecessary dead weight, amounting to many millions of dollars per annum. We will now compare the two gauges, in reference to the economical transportation of freight, when the cars are loaded to their capacity.

The average weight of modern broad-gauge cars is 20,000 lbs., capacity 20,000 lbs. The average weight of Southern broad-

Gauge.	Weight of box cars in lbs.	Capacity of box cars in lbs.
Broad	18,500	20,000
Narrow.....	8,000	16,000
	10,500	4,000

Average pounds of dead weight to one ton of paying freight carried on railways in Massachusetts and New York in 1870.

Massachusetts..... 3,136 | New York..... 3,109

gauge cars may be put down at 18,500 lbs., capacity 16,000 lbs. In the table we have taken the lowest dead weight, and the highest capacity, as we prefer to overestimate

the capacity of the broad gauge, rather than underestimate it. As will be seen by this table, the railways of Massachusetts and New York average only about one-sixth of their capacity. It should be borne in mind that the average of our railways will fall far short of those of the States referred to. If, as shown, Massachusetts hauls 18,500 lbs. of dead to transport 3,136 lbs. paying freight, and New York 3,091 lbs., what may we reasonably expect is the average in the agricultural districts of the South, West, and Northwest? The proportion of dead weight to paying freight on Southern railways is even much greater than it is in the West and Northwest. In a word, their railways have a capacity far in excess of the business of that section, entailing high rates upon them and thus retarding development which cheap narrow-gauge roads with their low rates would stimulate into great activity. It will be observed that the average of freight carried per car by Massachusetts roads is less than one-fifth the capacity of the narrow-gauge box car.

Way Freight.—In transporting quantities less than a full car load, the narrow-gauge cars have immensely the advantage. Cars carrying through freight may be loaded very generally to their capacity, but the average net car loads are brought by the necessity for dropping cars from the trains at turn-outs along the route with way or local freights, frequently very small in proportion to the full car load.

The following table shows the amount of dead weight per ton, in pounds, by the two systems, when the shipment of freight to a way station is not an even car load. The figures in the heading show the amount of supposed shipment in tons:

Dead weight per ton in lbs. on the two gauges.

Gauge.	Number of cars to carry shipment.	TONS. 1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
		DEAD WEIGHTS.										
Broad	1	18,500	9,250	6,166	4,625	3,700	3,083	2,642	2,312	2,055	1,850	.....
"	2	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	3,363
Narrow...	1	8,000	4,000	2,666	2,000	1,600	1,333	1,142	1,000	.....	.....	.....
"	2	.....	.....	.....	.....	.....	.....	.....	.....	1,777	1,600	1,454
Difference in favor of narrow gauge.....		10,500	5,250	3,500	2,500	2,100	1,750	1,500	1,312	278	250	1,909

This table shows that if it is necessary to drop cars with 9 tons of freight, at a way station—this being an amount which is one ton over a car load for the narrow gauge, and which renders necessary the use of two cars at the utmost disadvantage—the dead weight is even then only 1,777 lbs. per ton, and still 278 lbs. less than the broad gauge. These operative comparisons would seem to be sufficient to settle the question so far as the economy of the narrow gauge is concerned; but it may be well to look into the question of the reduced wear and tear, in consequence of the use of light machinery and rolling stock. If we assume that the repairs of machinery and rolling stock are in direct ratio to its cost, the reduction for the narrow gauge would be about 50 per cent., which is the difference in the first cost.

Comparative Strength of Rolling Stock.—The expenditure of power and ordinary repairs should be held strictly correlative; and yet there can be no doubt but that all the parts of the narrow-gauge machinery and rolling stock being relatively stronger, and the shocks to which they are exposed being so much lighter, both cars and locomotives will run with much less repairs than has been generally estimated. Narrow-gauge locomotives weigh from 6 to 18 tons, depending upon the nature of the service they have to perform. The following table gives the principal dimensions and

weights of various patterns and sizes of narrow-gauge locomotives, together with the loads they will haul on a straight track in good condition:

Dimensions, weights and loads of narrow gauge locomotives.

Kind of locomotive.	Cylinders.		Diameter of driver.	Weight in working order.			Load in gross, cars and lading.			
	Diam.	Stroke		Total.	On drivers.	On each pair of drivers.	On a level.	On a 40 ft. grade.	On an 80 ft. grade.	On a 100 ft. grade.
Class 1.										
Four wheels connected tank locomotive	9	12	30	18,000	18,000	9,000	390	120	70	55
	9	16	36	22,000	22,000	11,000	490	150	85	70
	10	16	36 to 40	26,000	26,000	13,000	590	180	105	85
Class 2.										
Four wheels connected with separate tender	9	12	30	16,000	16,000	8,000	385	115	65	50
	9	16	36	20,000	20,000	10,000	480	140	75	60
	10	16	36 to 40	24,000	24,000	12,000	580	170	95	75
Class 3.										
Six wheels connected tank locomotive	10	16	36	28,000	28,000	9,333	615	185	105	85
	11	16	36	33,000	33,000	11,000	740	225	130	105
	12	16	36 to 40	38,000	38,000	12,666	860	265	150	125
Class 4.										
Six wheels connected with separate tender	10	16	36	25,000	25,000	8,333	605	175	95	75
	11	16	36	30,000	30,000	10,000	730	215	120	95
	12	16	36 to 40	35,000	35,000	11,666	850	255	140	115

Here let us call attention to a matter that bears directly upon the question of "wear and tear," giving a table showing the weight on each wheel of ordinary trains of the two gauges, and the momentum with which the wheels will strike irregularities in the track.

Weight on wheels—their momentum.

[Passenger trains—speed 25 miles per hour.]

	Five Feet Gauge.		Three Feet Gauge.	
	Weight on single wheel, pounds.	Momentum, pounds.	Weight on single wheel, pounds.	Momentum, pounds.
Engine driver	8,000	288,000	6,250	225,000
Engine truck	5,000	180,000	2,500	90,000
Tender	7,650	174,000	3,500	126,000
Baggage car	4,000	144,000	3,000	108,000
Passenger car	4,760	171,360	1,780	64,080

The ends of the rails are beaten to pieces, the surface abraded, the ties splintered, the fibre of the wood cut under the iron, weak joints rapidly made worse, so that each succeeding wheel falls with an increasing force upon the ends of the yielding rails, by the tremendous forces developed by the passage of these enormous weights at high speed. The lightest broad-gauge coach weighs about 16 tons, or 32,000 lbs. empty, and hammers the rail joints with 4,000 lbs. on each wheel. When loaded and driven over the rails at 25 or 30 miles per hour, the weight of the blow is enormous and terribly destructive to the superstructure, crushing out the best rail in five or six years. The passenger car of a three-feet gauge would only hammer the rail with 1,500 lbs. per wheel. The same truth applies to locomotives.

A 30-ton locomotive, and its loaded tender, weighing about 17 tons, or a total of 47 tons, will exert a pressure of nearly 6 tons on each driving wheel. When

driven at a high speed, the strain upon the track is terribly destructive.

The "Fairlie" engine, constructed for narrow-gauge lines, bears its whole load, including wood and water, on the driving wheels, thus utilizing the whole weight in the work of hauling the train. Instead of an engine carrying 47 to 50 tons to obtain the power of 20, we have an engine weighing 20 tons and no more; and this load distributed over eight wheels, with a pressure of 2½ tons per wheel, instead of nearly 6 tons, as with the broad gauge. The action upon the rolling stock is the same as upon the track. The wheel receives a blow of precisely the same weight as that which it administers to the rail at a low joint, and the shock is transmitted to the axle, except what is taken up by the springs, and the yielding of the parts of the whole structure of the engine or car. The saving of dead weight transported is so much saved from the grand total of this destructive agency; and, by the reduced weight upon each wheel, no single blow of such enormous force can be given on the narrow gauge.

Classification of Expenses.

	Percentage of whole Operating expenses.	Percentage saved by Narrow Gauge.
Maintenance of Roadway—		
Repairs road-beds	.166	.055
Cost of iron for renewals	.129	.065
Repairs, building fences, etc.	037	....
Taxes	.038	....
Repairs of machinery and cars	.20	.070
Operating—		
Office expenses, agencies and employés on trains and at stations	.123	....
Fuel, oil and waste	.125	.041
Loss and damages to goods and persons, general superintendence, etc.	.030	....
Contingencies	.052	.017
Total	..	.248 p. c.

We leave this point with a single illustration, showing the total of the forces expended upon a defective joint on one side of the track, by the passage of a train of 50 passengers, on either gauge. The joint is supposed to have yielded, so that the wheels fall into it with a perceptible shock. In an example, selected for calculation, it results that the wide-gauge train will have inflicted a series of blows, amounting, in the aggregate, to 6,891,840 lbs., while the narrow train will have expended a force amounting to only 3,248,640 lbs. Fuel, oil, and waste are also reduced in cost in proportion to the power developed. The list of employees, cost of general superintendence, office expenses, loss and damage, are not estimated. The percentage of saving in the different departments may be estimated as in above table of expenses.

The narrow gauge may be operated for about 25 per cent. less than the broad gauge, and where the proportion of expenses to gross receipts is 70 per cent., the ratio of the narrow-gauge expenses to receipts would be 52½ per cent., and with strict economy probably as low as 45. The expenditure of power stands in the relation of about 35 to 54 in freight and 11 to 30 in passenger traffic.

V.—CAN NARROW-GAUGE LOCOMOTIVES BE CONSTRUCTED OF SUFFICIENT POWER AND SPEED TO ANSWER THE GENERAL REQUIREMENTS?

Your Committee most unhesitatingly answer yes; and for the following reasons:

PRACTICAL FACTS.

(1.) Because they are doing it daily, and any one who wishes to investigate the question has only to visit the roads where they are in use and witness their performance.

(2.) From the fact that the great standard-gauge roads are taking their heaviest freight engines off their lines, as fast as it can be done with economy, or as fast as they are worn out, preferring to run more trains rather than the extra heavy ones, because the breakage of draft irons, links, bumpers, and the fearfully costly lamination of the rail, etc., etc., amounts to more than the cost of the additional men and trains.

(3.) That by adopting the proper form of construction, the engines can have sufficient power to handle any number of cars that can be prudently and economically run together in one train, and that such a train can be handled with as much safety on the narrow as on the broad gauge; while, the proportion of dead weight being much less, the same number of train men will handle more tons of paying freight, on the nar-

row than on the broad gauge when worked up to the same tonnage.

(4.) All narrow as well as broad-gauge advocates acknowledge that trains can and are run too fast for the best interest of the roads and the public, and that it taxes them far beyond the bounds of economy to run fast trains.

(5.) That there is no difficulty in making as fast time with the narrow-gauge locomotive as the great majority of the broad-gauge road make, which is all the public demands; and in advocating economy in this particular, that we represent the true interests of the public.

(6.) That the locomotives of the Denver and Rio Grande Railway, freight and passenger, are giving entire satisfaction both as to speed and power.

RESULTS ON DENVER & RIO GRANDE.

A 17-ton freight locomotive on that road has hauled a train of 24 cars with ease up a grade seven miles long, averaging 40 ft. to the mile; four miles of the same having a grade of 75 ft. per mile.

Gross Weight of Train.

4 empty 8-wheeled box cars, 8,000 lbs. each	....	32,660
12 empty 8-wheeled platform cars, 6,000 lbs. each	....	72,000
8 loaded 8-wheeled platform cars, 6,000 lbs each	48,000	
And load on same, 16,000 lbs. each.	128,000	
		176,000
		280,000

pounds or 140 tons; and this over a new track. Add the weight of the locomotive, 17 tons, and we have a total gross tonnage of 157 tons. A 12-ton passenger locomotive has hauled the following load up the same grades on schedule time, viz.: 15 miles per hour.

5 platform cars, 6,000 lbs each	30,000
And load (rail) on same, 16,000 lbs. each	80,000
Total	110,000

lbs., or 55 tons. Add weight of locomotive and we have a total of 67 tons. But a still more remarkable performance was that of one of its passenger locomotives, that ran 181 miles with only 2,340 lbs. of coal, hauling the usual train of one baggage car and two coaches. Of this distance, 102 miles were run up an average grade of 40 ft. per mile, and 8 miles of 75 ft. per mile. The passenger trains on that road have frequently been driven at a speed of 30 miles per hour with entire safety, and with less oscillating motion than is observable on broad-gauge roads newly constructed. The construction and practical operations of the Denver and Rio Grande Narrow-Gauge Railway have been carefully examined by experts, and among them numerous broad-gauge advocates, who all agree that it is a complete success, and that the narrow-gauge locomotive has "sufficient power and speed to answer the general requirements."

VI.—CAN THE PASSENGER COACHES BE MADE SAFE, COMFORTABLE, AND POPULAR WITH THE TRAVELLING PUBLIC?

The passenger coaches of the European and American narrow-gauge railways are admirably adapted to the respective gauges, of varying proportions and capacity; all agreeing in the most important point, namely, that the proportion of dead weight hauled for each passenger is very much less than the broad-gauge.

DETAILS OF PASSENGER CARS.

The first-class narrow-gauge coaches constructed by Messrs. Jackson & Sharp, Wilmington, Del., and in use on the Denver and Rio Grande Railway, are 40 ft. long, over all, 7 ft. wide inside; 7 ft. 6 in. high; with two 4-wheeled trucks, wheels 24 in. in diameter, weight 12,000 lbs., and carry 36 passengers each. The sills are only 27 in. above the rail, making the centre of gravity very low; hence the cars ride exceedingly steady and with less lateral or oscillating motion than is usually observable upon the broad-gauge. The seats are arranged, double on one side, and single on the other, one-half the length of the car having the double seats on the right, and the other half having them on the left, so as to distribute the weight equally. The single seats are 19 in. wide or long; the double 36 in.; the aisle 17 in. If found desirable there can be no serious objection to increasing the width of the car to 8 ft., making the single seat 22 in., the double seat 39 in., and the aisle 23 in. These cars are finished in the best style; the wood-work, the upholstery, decorations, and the whole arrangements being strictly first-class.

For one to enter these cars is to be convinced that the traveller need have no apprehension as to his safety or comfort. In a word, your Committee find that it is the opinion of those best informed, that they furnish

every requisite called for by the first-class coach. While they will not furnish four seats to a passenger *who pays for only one*, they will furnish more passengers with a *preference* of seats than the broad gauge double seats do, while the seats are in every respect as comfortable. The cars being smaller, railway officials can equalize the seats to the number of passengers much better than with the broad-gauge, thereby saving largely in dead weight, wear and tear, and the consequent cost of maintenance. Sleeping coaches with a single berth on each side, can be constructed so as to be as comfortable as those now in use, and at the same time have the ratio of dead weight very much in favor of the narrow-gauge. In view of the *practical* facts enumerated, your Committee unhesitatingly affirm that the "narrow-gauge passenger coach" is as "safe and comfortable" as the broad-gauge, and that this being the case, they will become "popular with the travelling public;" especially as narrow-gauge railways from this small cost, can and will furnish lower rates of fare than the broad-gauge.

VII.—CAN FREIGHT CARS BE CONSTRUCTED OF CONVENIENT SIZE FOR THE TRANSPORTATION OF COTTON, LIVE STOCK AND GENERAL FREIGHTS?

The Denver and Rio Grande Railway are now doing a general freight and passenger business, and are carrying live stock, wool, lumber, and in fact every class of freight; and their officers give it as their unreserved opinion, founded upon actual experience, and which is concurred in by connecting roads, that they gain in every case where the size of the car comes in question, and that in no case is the extra room of the broad-gauge car equal to the loss in dead weight. This applies to every class of cars on the roads with which the Denver and Rio Grande connects. They carry 9 of the largest cattle in a car weighing less than 8,000 lbs., while the broad-gauge carry only 14 of the same class in a car weighing from 18,000 to 20,000 lbs. The stock cars have 4-wheeled trucks, are 24 ft. long, the door being at the side, but near the end instead of the centre, and on the opposite side of the other end. For heavy and valuable cattle, they have two gates in the car (which when not in use are folded back against the side of the car), which makes three rooms 6x8 ft., into which they put three head of stock each, giving a space of 2x8 ft. to each head.

The broad-gauge puts 14 head of the same cattle into a 28 ft. car, which gives the Denver & R o Grande and other narrow-gauge the same floor room that the broad-gauge has, and with much less dead weight.

Comparative dead weight in the transportation of cattle by the two gauges.

Gauge.	Weight of car in lbs.	No. of cattle per car.	Weight of cattle in lbs.	Gross weight of loaded cars.	Total weight per head.
Broad...	18,000	14	19,600	37,600	1 285
Narrow	8,000	9	12,600	20,600	888
Dead weight in favor of narrow gauge ...					397

A difference of 397 lbs. per head, 3,573 lbs. per car load of 9 head, and in a train of 20 cars 71,460 lbs. or 35 tons in favor of the narrow gauge. Prominent stock men now say that they would prefer to send their stock to market in such cars as the Denver & Rio Grande furnish, because it does not put so many together, and that the danger of cattle getting down is much less, while they can be fed and attended to much better. We will treat of the transportation of cotton under the next head.

VIII.—WHAT SAVING IN DEAD WEIGHT WILL THE NARROW GAUGE EFFECT?

Under this head your Committee desire to call attention to the fact as set forth by Mr. Fairlie, namely, that the East India narrow-gauge cotton car, weight 3,000 lbs., carries 10,800 lbs. of cotton or paying freight, while the Southern broad-gauge box car, weight 18,500 lbs., carries only 8 tons, or 16,000 lbs. Assuming that the cotton trains on our Southern railways average 25 cars per train, let us see how the narrow gauge will compete with the broad gauge in the transportation of the great staple of that section when moved in the standard English cotton car, and in order that we may be able to load the broad-gauge train to its full capacity, we will assume that the cotton is compressed:

Comparison of cotton trains.

Gauge.	Weight of single car in lbs.	Number of cars in train.	Total dead weight of cars in lbs.	Total paying weight in lbs.	Gross weight—broad gauge train in lbs.	Gross weight—narrow gauge train in lbs.	Excess of dead weight on broad gauge over the narrow gauge in lbs.
Broad	18,500	25	462,500	400,000	862,500	.....	.....
Narrow	3,000	37	111,000	400,000	.....	511,000	.....
Difference in favor of narrow gauge					.....	.....	351,500

A difference in favor of the narrow gauge of 351,500 lbs., or 175 tons.

We will now see how they compare when loaded with uncompressed cotton, which is the condition in which it is usually shipped. The common broad-gauge box car of the South has a floor area of 27×8 ft. $= 216$ sq. ft., weighs 18,500 lbs., and carries 24 bales, weighing 450 lbs. each. The paying freight carried per car is 10,800 lbs., and the dead weight 18,500 lbs. Or for each bale weighing 450 lbs., we have 770 lbs. dead weight. The narrow-gauge box car has a floor area of 162 sq. ft., weighs 8,000 lbs., and will carry 18 bales cotton weighing 8,100 lbs., or 444 lbs. dead weight per bale, against 770 lbs. by the broad gauge.

Comparison of trains with uncompressed cotton.

Gauge.	Weight of single car in lbs.	Number of cars in train.	Total dead weight of cars in lbs.	Total paying weight in lbs.	Gross weight—broad gauge train in lbs.	Gross weight—narrow gauge train in lbs.	Excess of dead weight on broad gauge over the narrow gauge in lbs.
Broad	18,500	25	462,500	270,000	732,500	.....	.....
Narrow	8,000	34	272,000	270,000	.....	542,000	.....
Dead weight in favor of narrow gauge					.....	.....	190,000

A difference in favor of the narrow gauge of 190,500 lbs. or 95 tons. We find that the narrow gauge has largely the advantage in all cases, and that

The narrow-gauge eight-wheeled box car.—Weight 4 tons, capacity 8 tons, or a total of 12 tons, weighs when loaded only 2 tons more than the heaviest broad-gauge box car when empty.

The narrow-gauge eight-wheeled stock car.—Weight 4 tons, capacity 9 head (largest cattle), or 12,600 lbs., weighs when loaded, only 600 lbs. more than the heaviest broad-gauge stock car when empty.

The narrow-gauge eight-wheeled platform car.—Weight 3 tons, capacity 8 tons, or a total of 11 tons, weighs when loaded, only 2 tons more than the heaviest broad-gauge platform car when empty.

The narrow-gauge eight-wheeled passenger coach.—Weight 6 tons, capacity 36 passengers, or in round numbers a total of 9 tons, weighs 6 tons less than the average of broad-gauge coaches empty.

Further argument on this point would seem unnecessary.

IX.—HOW WILL THE SAVING IN FIRST COST AND DEAD WEIGHT AFFECT THE RATES OF FREIGHT AND PASSAGE?

We have, at this time, about 60,000 miles of broad-gauge railway in operation in the United States.

Cost of the Present System.—These railways have cost from $20,000 to $60,000 per mile. Estimating their average cost at $40,000 per mile, they represent $2,400,000,000. Had we originally adopted the

narrow-gauge at one-half the cost of the broad, the expenditure would have been $1,200,000,000, leaving an equal amount for investment in manufactures or other means of development. Or with the $2,400-000,000 invested in the narrow gauge, *it would have given us* 240,000 *miles at* $10,000 *per mile, or* 120,000 *miles at* $20,-000 *per mile; in active operation to-day.* It needs no argument to demonstrate if we had double the number of miles in operation that we now have, that our resources would be developed much more rapidly than is now being done. Neither does it need any argument to prove, had our railways cost us *only* from $10,000 to $20,000 per mile, that it would have cost much less to have operated them; and that our general interests would have been advanced in a corresponding degree.

The cheaper the first cost of the road, machinery and rolling stock, provided they be ample for the business offering,—the lower the rates, and the consequent rapid development of all interests. The great want of the age is cheaper transportation, and this we cannot have without cheaper railways.

Comparative Cost of Transportation.—The average cost of transporting freight by the broad gauge, may be estimated at 1½ cents per ton per mile, and on the narrow gauge one cent. One ton transported 200 miles, at 1½ cent per ton per mile, will cost $3, and at 1 cent, $2, a saving of $1, or 33⅓ per cent. in favor of the narrow gauge. Estimating the cotton crop of the Southern States at 4,000,000 bales per annum, and the average cost of transportation at $3 per bale, we pay $12,000,000 freight per annum on this one product alone. Admitting that the cost of transportation by the narrow gauge would be one-third less than by the broad gauge, it would effect a saving to the producers of $4,000,000 per annum; or, for a period of 25 years, $100,000,000, a sum sufficient to build 10,000 miles of narrow-gauge railway at $10,000 per mile, or 5,000 miles at $20,000 per mile. The East India Company, looking to the *extension* of the cotton culture in their territory, have projected 10,000 miles of narrow-gauge railway; and that too, in a country far more densely populated than ours, and offering a large general business. In addition to this, they are changing some of their broad gauge to narrow gauge, in order to reduce the cost of transportation on cotton, so as to be able to compete with us in the production of that staple. The East India cotton planter commands an almost unlimited supply of the *cheapest*, most patient and easily controlled labor in the world. This, with their system of cheap narrow-gauge railways, with its low rates of freight, makes them dangerous competitors; and it is a serious question, and one well worthy our earnest consideration, whether, with our dear labor and high broad-gauge rates, we can continue to maintain our supremacy in this branch of industry without cheaper transportation than we now have.

The Great Want of the West.—The great West and Northwest, with their vast agricultural productions, demand *cheaper* transportation than the costly broad gauge railways can possible furnish them. They demand transportation so cheap that points now so far distant as to be unable to ship their corn, and hence use it for fuel, will be able to enter the markets of the world with the products of their labor. The demand is a just one, and should be complied with; it is a want that the great masses of the people are deeply interested in—that of cheap food. Your Committee might with propriety discuss this question at great length; but, as it is *self-evident* that railways costing only from $10,000 to $20,000 per mile, can work comparatively cheaper than those costing fron $20,000 to $60,000 per mile, there can be no question of their effecting a *large saving* in both the "rates of freight and passage."

X.—BREAK OF CONNECTIONS.

The time of transferring freight need be very little, if any, greater than is now necessarily consumed in the inspection and repair of cars at intermediate points, which are sent over long lines. The shipper can well afford a slight delay, if thereby he saves largely in cost of shipment. Experience has demonstrated the cost of transferring freight in Great Britain to be about 2 pence per ton. The cost on the Canada frontier has been found to be, from many years, experience, 5 cents per ton. Considering it as a question of cost, it will evidently bear heaviest upon through freight, which has to be changed at each end of a line, at a cost of 5 cents per ton, or a charge of 10 cents per ton total. The average cost of transporting freight may be taken at 1½ cents per ton per mile on the broad guage; but on the narrow gauge, we find that we can trans-

port 25 *per cent. cheaper in actual working expenses.* One ton transported 200 miles, at 1½ cents, would cost $3.00, and a saving of 25 per cent. on this would be 75 cents; so that an expense of 10 cents per ton may be incurred in transferring freight, and still leave a balance in favor of the narrow gauge, or shipper, of 65 cents per ton, $6.50 per car load of 10 tons, or $162.50 for a train of 25 cars. Further argument is deemed unnecessary upon this point.

XI.—EXPERIENCE AND OPINIONS OF EXPERTS.

Reference has been already made to the practical workings of the Denver and Rio Grande Railway, as well as the convictions and opinions of its officers; all tending to show conclusively that the narrow-gauge has ample capacity for the business of any line on this continent. But we will quote the testimony of other professional authorities and experts. The Annual Report of the Festiniog narrow (1 ft. 11½ in.) gauge Railway, for 1869, shows that it transported 136,132 tons net, and 97,000 passengers that year. Gross tons hauled, exclusive of engines, 242,000.

The total receipts for the year 1869, were	£23,676
Expenditures	13,053
Net income	£10,623

or nearly 50 per cent.

In Norway, railways of the 4 ft. 8½ in. and of the 3 ft. 6 in. gauges have been constructed by the same engineers, and worked by the same manager for the Government, and the following statistics are taken from the Government returns, showing the average from six years' experience.

	Gauge 4 ft. 8½ in.	Gauge 3 ft. 6 in.	Difference in favor of the nar. gauge.
Cost of construction per mile	26,343	17,143	9,200
Receipts per mile (alike)	27,600	27,600	.....
Maintenance per mile	7,173	6,565	608
Locomotive expenses per mile	9,426	5,760	3,666

Mr. Spooner, Engineer and Sup't of the Festiniog Road, under date of December 1870, gives the following opinions:

"From my experience in working the 1 ft. 11½ in. gauge, I deduce the following, to show the sufficiency of the 2 ft. 9 in. gauge: (1.) That the cost in construction, in earthworks, bridges, tunnels, etc., depends on the gauge. (2.) That the cost of maintenance of rolling stock will be low, consequent upon the small weight on each wheel. (3.) That a speed 40 miles per hour can be run with ease and safety." Mr. Spooner's opinions are of special value, as the study of this subject has been his occupation during ten years; and, as his experience has been more full than that of any other person, so is his knowledge of the matter more profound. In June, 1869, the British Government appointed a commission of engineers and railway experts to consider what gauge should be adopted for the Indus Valley and other projected railways in India. We quote from the majority report: "It is manifest that lines on this gauge (3 ft. 6 in.) are worked with as much convenience to the traffic as on any broader gauge, and vehicles may be used on this gauge which will afford complete accommodations for all classes of passengers and goods traffic.

XII.—THE NARROW GAUGE AS COMPARED WITH THE BROAD GAUGE AS THE MEANS OF DEVELOPMENT.

This is a question of vital importance, and one that affects all interests alike. This being the case, any system that will afford ample facilities for transportation at reduced rates, is a public blessing. The necessarily high rates entailed by the excessive cost of the broad gauge, might have been avoided by the adoption of the narrow gauge with its small cost, light operating expenses, and low rates. A most striking instance is that of the Southern States, with their vast and inexhaustible supplies of coal and iron, unsurpassed for their purity and richness, and the ease and cheapness with which they can be mined and worked' but which, from the high broad gauge rates of transportation, remain locked up in their hills and mountains. A cheap system of railways, with its low rates of freight, would have long since developed those interests, enriching that section, and adding vastly to the general wealth of the country. The great West, and Northwest, with their broad prairies and vast plains, unequalled for their general productiveness and the ease with which they can be tilled, also feel the incubus of high rates of transportation entailed upon the products of their labor from the same cause.

The reduction of rates which would follow the general introduction of the narrow-gauge system, would add millions of dollars per annum to their incomes, and at the same time largely benefit the consumer. General development can only be accomplished by an extended system of railways; quick development can only be attained by *cheap* transportation. We may therefore conclude that the narrow-gauge railway is by far the best means for a *general* and *quick* development of our resources, for the following reasons:

(1.) Costing only about *one-half* as much as the broad gauge, it is within the means of all sections to build them; hence will enable them to avail themselves of railway facilities, where otherwise they will be compelled to dispense with them.

(2.) From their small cost, light operating expenses and small interest account, they will prove to be paying investments.

(3.) They will supply the great want of the age, cheap transportation.

(4.) Cheapening transportation, they will develop dormant interests more rapidly than our present costly structures, with their high rates, can possibly do.

(5.) Their general adoption in sections without railway facilities, will enhance the value of properties largely in excess of their cost.

(6.) Penetrating those sections, and rapidly developing their resources by low rates, they will bring a large new business to the broad-gauge roads, enabling them to reduce their rates, and thereby stimulate old, and develop new interests.

(7.) A failure to adopt the narrow gauge in the sections referred to will necessarily *defer* the construction of railways until such time as their means will admit of the more costly broad gauge, with its consequent high rates.

COMMITTEE.

W. H. Greenwood, Manager Denver & Rio Grande Railway, Denver, Col., Chairman.

E. Wragge, Chief Engineer Toronto, Grey & Bruce Railway, and Toronto & Nipissing Railway, Toronto, Canada.

T. H. Millington, Chief Eng. Mo. & Kansas Railway Co., Bolivar, Tenn.

A. W. Bell, of Porter, Bell & Co., Locomotive Builders, Pittsburgh, Penn.

D. E. Small, of Billmeyer & Small, Car Builders, York, Penn.

Wm. S. Auchincloss, Vice-President Jackson & Sharp Company, Car Builders, Wilmington, Del.

Lucient Scott, Vice-President, Kansas Central Railway Co., Leavenworth, Kansas.

Chas. H. Howland, Cairo & St. Louis Railway Co., St. Louis, Mo.

W. M. Kasson, Chief Eng. St. Louis & Western Railway, St. Louis, Mo.

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E. Hulbert, President, N. G. &. N. C. R. Co., Atlanta, Ga.

THE MANUFACTURE AND WEAR OF RAILS.

(*Continued from p.* 190.)

Captain H. W. Tyler had been accustomed for many years past to hear constant complaints against the manufacture of rails. It had been said that the manufacturers had lost the art of making rails—that the introduction of the hot-blast had enabled them to work up inferior ores—that it was impossible to obtain either good material or good workmanship—and that, in fine, good durable rails, such as were formerly supplied, were not to be got at any price. It was also said that rails were wearing out in this country in six months, in situations where they formerly lasted for years; and that those sent out of this country for foreign consumption were worse than those manufactured for home consumption. He had quite recently read a letter, from the other side of the Atlantic, from a gentleman of great experience, who did not believe that English manufacturers could now make durable rails, and who thought that there was no remedy but the employment of steel. Last summer he saw steel rails being laid down on lines in the United States, which cost, including transport and a very heavy duty, about £25 per ton. Only a few weeks ago, he had inquired into an accident in the Penge tunnel of the London, Chatham and Dover Railway, which was occasioned by the failure of a rail, certainly very badly made, the top of which had fallen away in six pieces, and he saw many near it failing in a similar

manner. During a recent inspection of the Grand Trunk Railway of Canada, he found that rails laid down within the last few years had been failing to an extraordinary extent, and that portions of the line relaid with new rails were obliged to be again renewed in from three to five years, while the older rails were still necessarily kept in the track.

Now he had been carefully considering this question, both in England and in America; and particularly with reference to Canada—where, in consequence of the severity of the climate, rails of good quality were still more required than in England. The road was there in a perfectly rigid condition, for, say five months in the year, and the thermometer sometimes—40 deg., with the frost penetrating 5 ft. into the ground. But under those circumstances the durability of the rails was very various; some of them lasting six times as long as others, under the same description of traffic. He found this was not a question of price; for, strangely enough, the cheaper rails had lasted longer than those for which a higher price had been paid, in some cases at the suggestion of manufacturers, to insure greater durability. The failures appeared to be irrespective of the specification, and of tests applied. The principal defect was undoubtedly imperfect welding; and those rails which had worn best were generally those which showed on their fractured sections coarse crystals, and what would be called inferior quality. He did not believe that all the defects of manufacture which had been witnessed or experienced were due to faulty specifications. In truth, the engineers had generally consulted the manufacturers in drawing up those specifications. The difficulty had been to obtain durable rails under any specification. And when the number of specifications was complained of, for he had heard a manufacturer saying the other day that he possessed copies of two hundred and fifty different specifications, it must be remembered that a different specification was necessary in the case of each manufacturer, and of each section of rail, and was allowable in the case of each engineer.

In regard to tests, he believed that harm had been done by requiring manufacturers to make rails subject to the test of too heavy a falling weight. He had heard rails spoken of as being extra good because they would stand 17 cwt. falling 25 ft. Such rails were no doubt defective in other essential qualities.

In answering the question how to get a durable rail, it must be first determined what was required in a rail. A rail had often enough been compared to a continuous girder; but it was much more; for while it was required, like a girder, to resist compression principally in its upper, and tension chiefly in its lower chord, it was further required while deflecting very slightly, to wear gradually away under traffic on its upper flange, and to resist such wear and tear to the utmost. He by no means agreed in comparing the action of the wheel and the rail to a hammer and anvil, because, when the rolling weights inflicted a blow, they did so with the intervention of springs. When the rails were shallow, or wanting in depth, the blows became more serious, and failures by crushing at the ends, even when the joints were fished, were more apt to occur; but when the section was deep, and deflection was immaterial, the fish was more perfect, and a rolling crushing action was the principal effect to be encountered.

In regard to the best form of rail, whatever the reason might be, it was certain that the double-headed section of rail was mostly adopted in England; while on the continnent of Europe and America, such a section was only exceptionally seen. There were two principal reasons for the adoption of the double-headed section: rails of that section had a broader base by means of chairs, and they might be turned so as to wear out four instead of two edges. But, on the other hand, there were strong reasons against their adoption. They were raised higher over the sleepers, having the chairs below them; an amount of metal was put into the chairs which might with greater advantage be put into the rails; they could not, as a matter of manufacture, be made equally durable and serviceable in both heads; and when turned, they were more or less defective, and unserviceable.

With the Vignoles rail, on the other hand, the engineer and the manufacturer might combine to make a hard durable head without fear of a rail that would break in the track, because they could at the same time use fibrous material in the lower flange; and with this section all risk of loose keys was avoided, and there was also the advantage of fewer parts.

It was a prevalent idea that the Vignoles rail was not a good form of rail for

traffic at high speed, but he could not himself see the objection. He believed, on the contrary, that when deep in section, with a good broad base, and laid on square cross-sleepers at a proper distance apart, it formed the safest permanent way that could be constructed for any speed.

In regard to quality, that to some extent depended upon the form. As he had already said, a harder and more brittle head could be used with the Vignoles than with the double-headed section. Any rail, whatever might be the quality, which would not crush from bad welding, would last for a considerable time. He had an aversion to steel-topped rails, knowing that the steel and the iron welded at different heats, and believing that under the most careful system of manufacture, a large proportion of steel-topped rails must, sooner or later, fail from the separation of the steel from the iron. He had seen numerous cases, in different situations, where the steel had come off bodily, and he conceived that even the steel tops, which were originally well united, must be liable, in consequence of being rolled out under the traffic, sooner or later, to separate. He therefore thought that the statement in the Table was deceptive. The general question of the relative advantages of steel, as compared with iron, granting that the manufacturers would still make good iron, was simply one of price. Every one must admit the advantage of a homogeneous rail, the structure of which rendered lamination impossible; but it was not every Railway Company that could afford to pay a high price for steel rails for other than exceptional parts of their line.

He believed that, while guarantees were good in their way, and particularly in England, when rails could easily be returned to the makers, there was one method which would be better in the end than specifications or tests, and that was the unreserved publication of the results. If all engineers would combine to collect and record the price, the wear and tear, and the durability of each sample of rails from the various makers, and to publish their records annually with the reports of the companies, the manufacturers would be subjected to a wholesome competition with each other, which could not but be attended with the best effects.

Mr. J. A. Longridge said that the first malleable iron rails were made by his father at the Bedlington Iron Works. There was a strong contest in those days as to the comparative merits of malleable-iron rails and cast-iron rails; and the principal objection against the malleable-iron rails was that they laminated and exfoliated. Engineers were then in favor of cast-iron rails in preference to wrought-iron, and among them was George Stephenson. The first wrought-iron rails were laid on a private railway for coveying coals to the Bedlington Iron Works; and to George Stephenson's credit it must be said, that when he saw that although they did bend they recovered their form again, he at once recommended their adoption, instead of cast-iron rails, although he had a pecuniary interest in the latter. The manufacturers could still make as good or better malleable-iron rails than were produced at that time, or when the London and Birmingham Railway was in course of construction; and if the manufacturer was not bound by absurd tests on the one side, and by a specification which might suit one iron but would not suit another, and the sort of rail that was wanted was clearly stated, an excellent rail could now be produced for a fair price. A weight of 3 cwt. falling from a height of 20 ft. on the rails was equal to an accumulated shock on the rails of 3 tons. Supposing the weight on a driving-wheel of a locomotive engine to be 7 tons, and that at a joint in the permanent way one rail stood $\frac{1}{4}$ of an inch above the adjoining one, then the velocity with which that would fall would accumulate a shock on the rail of $\frac{1}{7}$th of a ton, instead of 3 tons, as in the case of the falling weight. He thought, therefore, that such a falling weight was an absurd test. He did not deny that the dead-weight test would give a certain result; but he was sure that almost any rail would amply support whatever weight might come on it as a dead weight. He did not agree with Captain Tyler that the form of the Vignoles rail was advantageous, because crystalline iron could be put where it was in compression, and fibrous iron where it was in tension. That was very true if a rail was supported simply between two joints, but inasmuch as a rail was a continous girder, in a certain position of the load the top of the rail was in tension and the bottom of the rail in compression; therefore if fibrous iron was required in one part it was required also in another. Yet rails were all so amply strong, that it was not a question of the strength of a rail, but of its dura-

bility. Iron rails should not be condemned because they wore out, but an attempt should be made to reform the rolling stock. The enormous weight put on the driving-wheels crushed the rails; and he thought conical wheels fixed to the axles should be discarded, as being very injurious to the rails; the better system would be to have every wheel revolving independently. Mr. Cross had tried this plan on the St. Helen's line with perfect success. He did not approve of the conical wheel, because, even if the wheels were revolving independently, one portion of the wheel-tyre must be sliding. Take the ordinary cone of the wheel, and the ordinary width of its bearing on the rail as only $\frac{3}{4}$ of an inch; then with a train of 100 tons moving at the rate of 20 miles an hour, there would be a grinding action equal to $1\frac{3}{4}$d horse power; and if the train moved at the rate of 60 miles an hour, it would be equal to a grinding action of 5 horse power.* This must have a prejudical effect on the wear of the rails. If they were simply subjected to a rolling motion, they would last for an indefinite period; and he was sure it was mainly due to the conical form of the wheels that the grinding and wearing motion took place; and this was increased by putting gravel and sand on the rails when they were at all greasy. He thought that steel tyres, which were now being adopted, working on steel rails lessened the adhesion, and that the weight on the driving-wheels would have to be increased. Therefore he maintained that wrought-iron rails should not be discarded. The proper course to pursue was to reform the rolling stock, to distribute the weight on a greater number of driving-wheels, to have wheels revolving independently, and to abolish the conical wheel.

Captain H. W. Tyler observed that Mr. Longridge had stated that it was necessary to provide for tensile strain in the top flange as well as the bottom flange of a rail. Now, in any case in which a weight was applied on each side of a sleeper, the top flange of the rail no doubt would be more or less in tension; but in reality and in practice the distance between the engine-wheels, and especially the driving-wheels, was at least 5 or 6 ft., while it was commonly more; whereas the distance between the sleepers was only $2\frac{1}{2}$ ft. or 3 ft.; therefore there could never be a weight supported on both sides of any sleeper at the same time. When the rails were sufficiently deep, and when the sleepers were sufficiently near together, the deflection of a rail under passing loads was imperceptible; and any strains which could be produced in tension on the upper flanges of the rails over the sleepers were practically immaterial, and were not to be compared to the strains in tension on the lower flanges between the sleepers.

Mr. John Boyd thought that if engineers abolished tests they would not get rails as good as those now obtained; although the falling-weight test was perhaps excessive at the present time. He did not think the mode of manufacturing rails had been sufficiently considered. It was certain, that by a proper mixture of iron in the puddling furnace a better rail could be obtained than by making use of any one quality of iron; for instance, by a mixture of Cumberland "red short" hæmatite iron with Welsh iron, and also with Cleveland iron, both of which were "cold short." Sir Charles Fox once told him that he saw rails from England being delivered in America, and several of them broke when they were thrown down. He had formerly known the Scotch foreman who was superintendending the delivery, and he said to him, "What a pity it is your Railway Companies don't give £1 a ton more, and get better rails than these." The reply was, "Weel, Sir Charles, they are just as guid as the bonds we gie for them." Before giving a judgment on the wear of the rails, it should be known how these rails which only lasted three or four years were paid for. If engineers did not specify the mode of manufacture, they should at least specify the description of iron of which the pile should be formed; and he would suggest that the top and bottom slab of the whole length and breadth of the pile should be formed of No. 2 hammered "all mine-refined" iron, and the intermediate slabs of the whole length of the pile laid so as to break joint, of puddled bars, and that the quantity of cinder in them should never exceed one-sixth of the whole burden on the furnace, and that no crop ends should be used, and the thickness of the

* The formula for this was—

$$\text{Horse-power} = \frac{w\,v\,b}{33{,}000\,f\,.\,\pi\,D}$$

When w = total weight of train in lbs.
" v = velocity of train in feet per minute.
" b = breadth of surface of tyres in contact with the rail.
" $\frac{1}{f}$ = coefficient of friction.
" $\frac{1}{\pi}$ = degree of conicity of tyres.
" D = diameter of wheel in feet.

slabs be so arranged that all parts of the pile should be reheated simultaneously.

He was of opinion that moderate, and particularly falling tests should not be discontinued; and that care should be taken that the bulk of all the rails really were equal to the quality of the tested rails; as it was notorious that rails said to have stood the test stipulated for by the East Indian Railway Company's Engineer at the maker's works had not borne the same tests at Rotherhithe; and in his opinion the reputation of engineers and the safety of the public required that tests should not altogether be abolished.

He submitted the analysis of West Cumberland hæmatite Bessemer pig iron, which was extensively used by the makers of Bessemer steel throughout the kingdom for rails and other purposes. He advocated the use of a mild steel rail, as having most adhesion.

"REPORT UPON THE WEST CUMBERLAND 'BESSEMER' PIG IRON, BY DR. NOAD, F. R. S.

"LABORATORY, KINNERTON STREET,
"*Dec. 10th*, 1867.

"DEAR SIRS,—I now have the pleasure of reporting on the last sample of Bessemer Iron which you sent me for analysis. I consider it to be first-rate iron; the quantity of silicon being by no means excessive, and sulphur and phosphorus wholly unimportant.

"I am, dear Sirs,
"Yours very faithfully,
"(Signed) HENRY M. NOAD.

"Messrs. WEST CUMBERLAND HÆMATITE
"IRON Co., *Workington*."

Analysis.

	per cent.
Graphite	3.850
Silicon	1 728
Sulphur	0 030
Phosphorus	0.044
Manganese	0 072
Titanic acid	0 152
Iron	94 124
	100.000

"REPORT UPON THE SAME BY DR. TOSH.

"CHEMICAL LABORATORY, MARYPORT,
"*Dec. 9th*, 1867.

"The specimen of pig iron received on the 27th ult. has been found to have the following composition:—

		per cent.	
Carbon	as graphite	3.989	4.485
	combined	496	
Silicon		1.767	
Sulphur		.008	
Phosphorus		.025	
Titanium		trace.	
Manganese		.219	
Iron		93.724	
		100 228	

"From the above results I have no hesitation in saying that this iron is of excellent quality.

"Considered with regard to its adaptability for Bessemer steel making, it is in every respect one of the best irons which I have examined.

"(Signed) EDMUND G. TOSH, Ph. D.

"Messrs. WEST CUMBERLAND HÆMATITE
"IRON Co., *Workington*."

Mr. F. D. Banister gave some details of the wear of rails on the Brighton Railway, and illustrated his remarks by a tabular statement. On the first 123¾ miles of the line from the London end towards Brighton, which carried the South Eastern and the Brighton main and local traffic, a length of seven miles being a quadruple, and the remainder a double line, the total expenditure in rails for ten years, from 1856 to 1866, had been £20,865, or £1591 3s. 10d. per mile per annum; giving a percentage on the original cost of £12.11, and showing a life of 8.10 years. In the suburban district, from Victoria to Norwood, on the West End and Crystal Palace line, the expenditure per mile per annum was £77; the total expenditure in ten years, £8,097; the percentage of these on the original cost per mile per annum, £9.31, and the life of the rails, 10.73 years. On the remainder of the main line, from Red Hill to Brighton, the expenditure per mile per annum was £52 7s. 4d.; in ten years, £21,712; giving a percentage of wear of £8.53 per mile per annum, and a life of 11.71 years. On the coast lines, Brighton to St Leonards, and Brighton to Portsmouth, the results were nearly the same—expenditure, £40 per mile per annum; total expenditure, £15,000; and a percentage of £4.94 per mile per annum on the original cost. There were some curious results on local lines. On the line between Wimbledon and Croydon the expenditure in rails in ten years has been £11 6s. 8d. per mile; total expenditure, £511. On the coast line, from Lewes to Newhaven, which had been in use for twenty years, the expenditure for the last ten years had been only 14s. 10d. per mile per annum for rails. That showed that those rails were remarkably hard;

LONDON, BRIGHTON, AND SOUTH COAST RAILWAY.

Average cost per mile per annum, and other details respecting the maintenance of way and works, for ten years, ending 31 December, 1866, including renewals both of way and bridges, but exclusive of stations and signals. Materials net.

Length Miles.	Length Chains.	From	To	Wages.	Rails.	Sleepers.	Chairs, etc.	Renewal of Bridges.	Total.	Original Cost of Rails £7 5s., less cr. for old £3 12 6. Net Cost £3 12 6.	Amount expended in rails in ten years, net.	Life of rails in years.	In ten years' wear of rails, per cent. on net cost.	Tonnage per annum.	Remarks.
				£. s. d.	£. s. d.	£. s. d.	£. s. d	£. s. d.	£. s. d.	£.	£.				
12	65	B. Arms, Junc.	S. Nest.....	374 18 5	159 13 10	55 6 2	49 7 8	11 0 0	650 6 1	17,100	20,865	8.19	12.20	2,096,640	Seven miles quadruple, the remainder double line. Average speed 35 miles per hour.
29	60	Red Hill	Brighton....	172 3 4	72 7 4	59 12 8	51 14 8	....	355 18 0	25,436	21,712	11 71	8.53	898,560	Double line Speed 35 miles per hour
32	53	Brighton	St. Leonard's	121 16 4	42 11 8	43 13 3	38 18 9	56 9 8	303 9 8	27,926	13,844	20.17	4.95	318,240	Ditto. Ditto 30 ditto.
41	43	Ditto ..	Portcreek...	105 1 10	38 0 6	50 14 3	38 10 0	44 9 6	276 16 1	35,514	15,966	22.24	4.49	555,440	Ditto. Ditto 30 ditto.
9	27	Croydon	Epsom.....	143 2 2	79 15 7	56 6 8	31 13 4	....	310 17 9	7,983	7,169	11.13	8.98	861,120	Ditto. Ditto 30 ditto.
10	13	Norwood	Victoria	332 5 8	77 0 11	31 8 7	17 8 7	....	458 3 9	6,688	8,097	10.73	9.31	3,781,920	Ditto. Ditto 30 ditto.
			Average..	208 4 7	78 4 11	49 10 3	37 18 10	18 13 2	392 11 10	....	....	14.03	8.07	1,418,653	
6	4	Croydon	Wimbledon .	67 0 0	8 10 0	11 6 8	6 13 4	....	93 10 0	2,586	511	....	....	411,840	
9	9	Keymer	Lewes.....	157 2 2	9 8 10	43 13 4	18 4 5	102 2 0	330 10 9	7,791	858	....	....	336,960	
5	47	Lewes..	Newhaven..	103 9 1	0 14 10	37 16 4	10 10 10	79 5 5	231 16 6	4,777	48	....	....	299,520	

they had worn very well, but more broken rails had been found there than on any other part of the system. The general result of the ten years' wear was an expenditure for rails of £78 4s. 11d. per mile per annum, and a percentage on the original cost of £8.07; the average maintenance of works over that portion was about £392 11s. 10d. per mile; so that the percentage for wear of rails upon the £392 11s. 10d. was nearly 20, and the average life had been 14 years. He might mention that between the bridge over the Thames and the Victoria Station the rails originally laid by Mr. Fowler were of iron; they lasted two years, and had been twice renewed; but they had now been replaced by rails of steel. On a portion of the line near Bricklayers' Arms Station, in the summer of 1864, he had laid down 400 yards of steel rails, manufactured by Sir John Brown & Co. Over that portion on the down line the whole of the South Eastern and Brighton traffic had passed, being 16,500,000 tons, in 3 years and 8 months. Those rails had been carefully measured, and he found that the wear had been a little more than $\frac{1}{16}$th of an inch. The iron rails adjoining were worn out in about 16 months, with a traffic of about 6,000,000 tons, the speed in both cases being 30 miles an hour.

Mr. W. Bridges Adams observed that in the question of rail manufacture, the engineer was the designer of the form, strength, and quality required, if he were a veritable constructive engineer, while the manufacturer was the producer of what would sell and make most profit. The ironmaster preferred pigs, "the sow and pigs," in cast iron; next, heavy round bars, then square bars, then the section of a parallelogram, then the parallelogram channelled to make a rail; but improved forms he called fancy iron, and eschewed them, if he could, till a sufficient pressure was put upon him by the fear of losing trade. The quality prized in Bessemer rails was homogeneity. Whether they were steel or iron was not a settled point. Mr. Bessemer himself did not decide whether they were steel or iron. Probably they were steel, from the fact of their breaking uncertainly, and, as shown by a sample on the table, the same bar breaking within a foot of the upper edge, where it was turned down under the engine-wheel as though it were a piece of lead. A good rough test for steel would be hardening and tempering a portion for a spring. Steel must be used either hardened and tempered equally, or annealed throughout. If neither of these processes were resorted to, it would be irregularly hard and soft, and would infallibly break. This was probably the reason why the maxim had grown up—"keep them soft." Bessemer rails in their best form were homogeneous iron, and would of course rub away under heavy friction, like any other homogeneous iron equally soft; but they would not laminate, as iron—not being homogeneous—was liable to do. Iron rails were made by what was called piling, *i. e.* iron in short lengths of differing quality, covered over with scale, everywhere except at the ends, where they were not intended to be united. These piles were full of hollow scale-cells, to which the air got access while in the furnace, and when rolled out they were a mere series of fibres and ribands, separated by scale, in straight lines; and if farther rolled out, they would still be straight fibres, and not gnarled in curves like the grain of oak or elm, or like tinplate or sinuous iron, or like double thin steel, piled and curved and re-piled, or Damascus sword blades, or the original puddle bloom from the furnace. It seemed desirable with single-headed rails to obtain a hard granular top and a fibrous bottom to the rail, and it was possible to obtain blooms of those opposite qualities at the pleasure of the puddler. There was a process of uniting two such blooms without any scale between them, and therefore of rendering them homogeneous. They required to be flattened under the hammer into oblong slabs, with good surfaces and squared edges, and then to plane the two surfaces which were to be united to a close fit. Placed together, and heated in the furnace to a welding heat, they would roll out homogeneously. But when the best rails were produced, the question arose as to the most judicious mode of applying them in line. A sample rail of the bridge form from the South Wales line, after being down for many years, appeared as good as ever. But it had been laid on a continuous longitudinal timber, and was boarded on the top, to make the iron cross the fibres of the wood; for it had been found in practice that with the rails parallel to the fibres the longitudinal logs split. The rail was perfectly granular, and had it been used with intermediate chairs, it would probably have broken. Chairs formed anvils to rails, and notched them below while they were bruised above by the wheels.

Neither had fish-joints yet been perfected. Some elasticity must be given, not positive play, but a vibratory elasticity, and a true joint connection.

Increased speeds had to be provided for, multiplied threefold for 7 and 8 tons load on each driving-wheel, instead of two, and for increased length of wheel-base growing from 7 ft. and 8 ft. to 16 ft. and 30 ft., and constantly increasing curves on the lines of rails. In short, long rigid parallelograms were being driven round very sharp curves, the wheels at a tangent with the rails. What the amount of grinding was might be gathered from the reversal of a double-headed rail. It at first presented a series of square-edged notches, ½ in. deep; at the end of a week the edges were taken off; in another week the metals became small curves, then large curves, and gradually the rail became a level plane. If the wheels rolled instead of sliding, they would simply roll over the curved surfaces; but as they slid, they became a planing-machine, and cut off the projecting metal. To get rid of the friction, it was necessary first to radiate the axles so that the wheels would always be in parallel planes to the rails, whether on straight lines or on curves, or double reverse curves. Then to form the carriages with curved spring-ends instead of side-buffer rods, to use short openings between the vehicles, closing the trains to lessen resistance, and prevent the vehicles riding on each other's backs in collision, to use continuous breaks, self-acting by gravitation, like steelyard levers—the normal condition to be pressing against the wheels, lifted by guard or driver, and instantaneously in action on inclines, in case of a coupling breaking. When these things were done, the engine-power might be reduced, and the weight of the engines or the load of the train might be increased. Tank engines were used, because they could run with either end forward. Tender engines, if properly coupled, making one bending machine of engine and tender, would do the same. At present, in order to obviate a vicious design, the driver screwed up the engine and tender rigidly together; and a long stiff body was thus formed, which was apt to get off the line, and which indeed would not keep on at all but for its weight. The question of light engines and vehicles was important; but when the surplus resistance was removed the trains might be lightened. Light trains should be used for light branch traffic; but with traffic unlimited, the loads on the engine driving-wheels should be what the rails and lines would bear without being crushed, and the train capacity should be in conformity. If tank-engines were used for long journeys, the load of water, which was a diminishing load, should not be placed near the driving-wheels, but as far away as possible. Any tender-engines could be altered to run tender foremost as steadily as a tank-engine; and two engines could be efficiently coupled together to work as twins, with water-tanks for short distances, without injuring lines or rails.*

Mr. E. A. Cowper had prepared a Table showing roughly the different modes of manufacturing steel and iron, simply for quick comparison of the number of operations in each of the processes. In the old practice the iron used always to be refined before being puddled, but that process was now generally omitted, from economical motives, though he was afraid in some cases it was but false economy, and that the frequent very short life of a rail was partly due to it.

According to the old process for making steel there was of course the cementing, the melting in pots, the hammering the ingots, and the rolling, in addition to the refining and puddling. The melting of steel in quantities of about 40 lbs. each in separate pots was a most expensive process, the wear and tear of the furnace on such small quantities was very great, and it was therefore impossible to make cheap steel rails on that plan, though it was admirable for tool-steel and other expensive kinds. He had put Lowmoor as the name of a process, although it was practised at other places.

The pigs were first taken and puddled to produce hard iron—iron with about .15 per cent. of carbon in it; the ball was then hammered thoroughly into a thick flat cake; it was not rolled at the same heat, but it was thrown on one side to cool. It was then broken up and selected, and he believed that the process of examining each piece and selecting the iron was one of the causes why it had won such an excellent name for tyres and for other things where iron was required to be hard. By these means it was known that every part was hard; there was no question of one puddler bringing out a soft ball and another a hard

* *Vide* "Railway Practice and Railway Possibilities," by W. Bridges Adams, London, 1868.

one, and the iron being all mixed or "piebald."

Then came what was commonly called the "Marshall" process. In this there was the smelting, the refining, the puddling, the hammering the balls, and the rolling the puddled-bars; then the "cleaning" of the iron, which was a species of "milling," and consisted in putting the small pieces of iron, cut into short lengths, into a cylinder, with a quantity of sand and water, and turning them round and round until they were thoroughly cleansed from all black scale; afterwards this bright iron was piled and rolled, and "blacks" in the iron were thus avoided.

Table showing the number of operations in each of the various processes for making iron and steel.

	Old Practice.	Modern Practice.	Lowmoor.	Marshall.	Krupp.	Bessemer.	Siemens.	
							Scrap.	Ore.
IRON.	Smelting. Refining.	Smelting.	Smelting.	Smelting. Refining.		Smelting. Melting in air Furnace. Blowing in Converter	Smelting.	Melting in open Hearth.
	Puddling.	Puddling.	Puddling H. I.	Puddling			Puddling.	
	Hammering Balls.	Hammering or Squeezing Balls.	Hammering. Breaking and Selecting.	Hammering Balls.		Hammering Ingots.	Hammering Balls.	
	Rolling P. B.	Rolling P. B.		Rolling P. B. Cleaning. Piling.			Rolling P. B.	
	Piling.	Piling.	Piling. Hammering.					
	Rolling.	Rolling.	Rolling.	Rolling.		Rolling	Melting in open Hearth. Examining. Hammering Ingots. Rolling.	Examining. Hammering Ingots. Rolling.
STEEL.	Cementing.				Smelting. Puddling. Rolling. Hardening. Breaking and Selecting. Melting in Pots.	Same as above.	Same as above.	Same as above.
	Melting in Pots.	Melting Cast and Wrought Iron in Pots.						
	Hammering Ingots. Rolling.	Hammering Ingots. Rolling.			Hammering. Ingots. Rolling.			

In Krupp's process of making steel, the metal was first puddled, then rolled into bars about 0.9 in. or 1.0 in. sq., and thrown into cold water, which, of course, hardened the steel from end to end. It was then broken up into short lengths, so that every piece of 3 cubic in. was examined at both ends, and it was examined and selected according to quality. The pieces were then put, like the cemented steel, into pots, melted, cast into ingots, hammered, and rolled. A sort of military discipline was exercised in bringing the men with the melted steel in the pots up to the bath, or pond of metal, by the sound of a bugle; and sometimes more than a hundred pots full of steel were got

together for one cast. Krupp, however, now used Bessemer steel largely; and he had many Bessemer converters at work, although he did not exhibit any steel at the Paris Exhibition by the name of Bessemer steel. Much depended upon the manganese and other ingredients put into the pots with the steel in this as well as in other places.

In the Bessemer process there was the smelting the pigs, the melting, the blowing, the casting, the hammering and the rolling. The blowing was the chief process, and by it either wrought iron or steel of various degrees of hardness could be produced. The use of a certain quantity of spiegeleisen was of great importance.

Last on the table were Mr. Siemens' two processes. In the first plan for making steel, from puddled bars or scrap iron and cast iron, the wrought iron, having of course being smelted, puddled, hammered, and rolled, was simply melted down in a bath of melted cast iron, in the hearth of a furnace having an intense heat, but no oxidizing flame, and the ingots were cast from this large bath of melted metal.

In Mr. Siemens' second plan the iron ore was brought into a state of "sponge of iron" by the heat to which it was subjected in a non-oxidizing atmosphere, or one in which some carbon was present, though not enough to convert it into cast iron; the iron was not in any way exposed to a "cutting flame" or "burnt," but the drops of metal were absorbed, or, so to speak, dissolved out of the "sponge of iron" by the small quantity of melted cast iron at the bottom of the hearth. There was, therefore, in this process only the melting, examining, the hammering into ingots, and the rolling; thus by either the first or second process there was obtained a perfectly homogeneous cast steel, or wrought iron, without any blacks or bad welds caused by piling or lapping up of cinder in the ball, as in puddling.

He thought Mr. Bessemer was quite right when he, some years ago, said that no perfect metal could be produced without its being melted; and now there was a furnace that produced such an intense heat, that tons of steel or wrought iron could be as perfectly melted as the little drops of steel of 40 lbs. weight were in the steel pot-furnace of former days, and yet without any burning of the iron or steel. He thought each of the processes named in the table depended greatly for its success on some one point being attended to. Thus, in the old process, "refining" was of great use in preparing pure "plate-metal" before puddling; while the practice of "breaking and selecting" in the Lowmoor and Krupp processes was admirable. The old plan of melting in pots was, and still continued to be, a very expensive process; whereas the Bessemer plan not only made steel and wrought iron very direct from cast iron, but gave the power of casting large ingots cheaply, though without the means of examining the metal in the process. In Mr. Siemens' plan there was certainly the advantage of being able to examine and test the metal under operation, and either to increase or diminish the quantity of carbon, so as to make "high" or "low" steel or wrought iron at pleasure; and from the casting of such large quantities of metal at a time, and the economy of fuel in the furnace, excellent metal could be turned out very cheaply.

Mr. F. Stileman stated, through the Secretary, that the total annual cost for maintenance and renewal of the permanent way of railways was above 9 per cent. of their gross receipts, and in the year 1866 amounted to £3,466,668. This expenditure showed an increase of £507,709, or 21.6 per cent. over the expenditure of the year 1863, the increased mileage for the same period being 13.22 per cent.

The estimated tonnage of rails laid in main lines was 2,438,520 tons; taking the life of iron at ten years, it would require 243,852 tons, at a cost of £7 per ton, equal to £1,706,964 to be expended annually; if renewed with steel rails, lasting double the time, 121,926 tons would be required at £11 per ton, equal to £1,341,186, showing a saving of £366,778 per annum, or nearly 1 per cent. of the gross railway receipts.

Mr Brunlees observed, through the Secretary, that the rails on main lines were, generally speaking, more deficient in bearing surface than in quality. Their tops were too narrow for the weight of engines now employed, hence they became laminated, and were crushed long before they were worn out. If rails were made with a top double the present width, or of the same width as the tyres of the wheels, the crushing power of the wheel thus diffused would be reduced to one-half, consequently the life of a rail would be doubled. To make such a rail, a weight of about 30 lbs. per yard would have to be added to the 80 lb.

rail; but the additional cost would only amount to one-third of the additional cost of laying down steel rails at 80 lbs. per yard. He had, therefore, no hesitation in saying that the saving which a wider topped rail would effect in the wear of tyres would alone very shortly pay for the additional weight of metal; whereas, if steel rails with only the ordinary width of head were used, the wear of tyres would be considerably accelerated. To maintain a good line, and a fair top on the rails, more lateral strength was required, and this would be gained in the wider rails. There would also be a considerable saving in maintenance, and that sometimes oscillating, sometimes trembling motion, due to the weakness of the road and the wear of the tyres, would be completely done away with. No doubt there were special places where steel rails might be used with advantage, but, generally speaking, iron rails of good quality and of greater section, combined with a judicious distribution of the weight on the locomotive wheels, would meet a greater number of present desiderata than could be supplied by the use of steel.

Mr. Joseph Mitchell expressed his opinion, through the President, that the flat-bottomed form of rail might be the best, if of steel, laid irrespective of expense, on a continuous bearing; but he doubted the superiority of that form of rail when laid on cross-sleepers, for it thus became, in fact, a girder between two points 3 ft. apart.

It was inferior as such to the double-headed rail, as it had less depth, being for a rail weighing 75 lbs. per yard about $5\frac{1}{4}$ in. in depth; whereas the double-headed rail was generally $5\frac{1}{2}$ in. deep, and was formed like a girder, with its effective strength on the upper and lower sides; the cross-bearing also was only 5 in., as compared with the bearing of the chair, which was 10 in.

If the flat-bottomed rail was made higher than $4\frac{1}{2}$ in., it was apt to deflect with the weight of the trains. No doubt it was the cheapest, but not much, the saving not exceeding £70 per mile.

Although the double-headed rail, with its numerous fastenings, still required improvement, yet, in practice, it had been found to be the best, or it would not have been continuously relaid, and otherwise so universally adopted by all the principal engineers throughout the kingdom up to this time.

Again, good ballast was an important element in securing a sound and cheap permanent way. If a line was not well ballasted and the ballast of good material, the rails would never wear equally, however superior might be the quality of the iron. If the ballast was soft or clayey, the pressure of the trains would make the rails sink in wet weather, and thus create irregularities which, with the repeated concussion of the engines, tended much to destroy and damage them. Now, on a large portion of the railways he had made in Scotland, extending over 280 miles, not only was there good gravel ballast, but the embankments and cuttings were mostly graved. The consequence was that the motion of the trains was so smooth and easy as to be a matter of general remark. The rails on the first section opened, which were double-headed, had been down thirteen years, and he had no doubt they would last six years longer.

He thought sufficient data had not been obtained to afford a satisfactory conclusion on this subject, and he suggested that permanent-way engineers might be asked to divide the lines under their charge into sections of 10, 20, or 30 miles, or such other distances as were subject to similar traffic; and to report on the nature of the ballast, the size of the sleepers, the form and weight of the rails—and, if possible, the makers' names, the average number of trains, and the average tonnage passing over those sections per annum; also the quantity and cost of ballast per mile, the cost of wages per mile, and the number of sleepers and rails renewed in each year.

By a return of this nature, an accurate estimate of the cost of the maintenance of permanent way under every variety of circumstances, might be obtained, and thus proprietors of railways might know, by comparing the maintenance of one railway with another, whether those they were interested in were maintained in the condition and at the expense they ought to be.

Mr. J. M. Heppel suggested, through the President, that rails should be tested directly for durability. Suppose, for example, a pair of rails 10 ft. long to be laid down, and a pair of wheels carrying 10 tons to be kept, by a small fixed engine with a crank motion, continually rolling backwards and forwards over them till they exhibited symptoms of failure. It seemed to him that this would nearly represent the actual condition under which the wear and tear

took place; and at any rate, as a comparative test between different specimens, would give very useful information. In this way in about a month an amount of tonnage might be passed over equal to that by which the best rails were worn out. No doubt the tendency of such a test would be to stimulate the production of hardness at the expense of ductility; and to counteract this, some test for the latter, such as a blow from a falling weight, would have to be added.

He thought that if manufacturers were requested to tender on their own specifications, but guaranteeing a certain resistance to impact, and a certain tonnage passed over at a given speed, in the way just mentioned, all their special experience would be utilized, and there would be a considerable amount of security for the result.

He was aware that some difficulty would arise in submitting to test a sufficient number of specimens without undue expenditure of time and money. Still it might be managed; and if the results to be expected were as valuable as they appeared to him likely to be, a considerable outlay in obtaining them would be well repaid.

Mr. W. M. Neilson suggested, through the President, that rails should not be tested at the works of the makers, but at a public testing-machine, under responsible management, to which rails might be sent for undergoing the test process. Experience would give those who managed this machine facilities for discovering the different qualities of rail submitted to them. Suppose four rails to be tested together, by four wheels hooped with ordinary tyres, suspended by a lever pendulum frame, the wheels to be loaded to the required weight, and so carried by a spring as to give such a shock to the rails as might be found desirable. The lower end of the lever, by touching projections in its motion, would cause the side oscillating motion; and the amount of adhesion might be measured by buffer springs at the ends of each of the rails. A simple attachment to a small steam engine would give the requisite motion to the machine, which would, of course, be kept constantly at work; the number of oscillations and pressures on the buffer springs being recorded by a tell-tale.

The motion of the wheels round the axis might be regulated by springs or weights, suspended by chains leading from pulleys over the centre of the pendulum frame; but the reverse motion of the wheels might not be a too severe element in the test.

Mr. Sandberg said the discussion had been such as to render any reply on his part almost unnecessary. However, he should like to direct attention, first to the steel-headed rails, and next to the Annuity Tables. His object in alluding to the steel-headed rails arose from the difficulty which existed in foreign countries in usefully employing the worn-out iron rails. In countries round the Baltic and the Mediterranean, they had been re-rolled with iron slabs, but of late Bessemer steel had been applied for the heads. He never expected to obtain perfect homogeneity, nor to get the same wear out of the steel-headed as out of the solid steel rails; and, therefore, in the annuity tables the former were represented as lasting only half as long as the latter. If the worn-out iron rails could be converted into solid steel rails by Mr. Siemens' plan, it would certainly be a great advantage for railways, not only in this country, but in the colonies, and indeed wherever far removed from the seat of manufacture. These steel rails would, of course, have to be equal to the Bessemer rails in quality and price, and if Mr. Siemens could succeed in making them as cheap and of equally good quality, he would not for a moment continue to defend steel-headed rails.

As to the annuity tables, some objection had been made to the 80 years taken as the life of solid steel rails. According to his view, compound interest was an important feature in arriving at a just comparison between the values of the three descriptions of rail, and he had no other means of expressing their relative capacities of enduring wear than by giving their life in years. He thought that sufficient experience had been adduced in this discussion from different railways, to prove that solid steel rails lasted on an average six times as long as common iron rails, though he admitted that iron rails could be made of a quality far superior to that usually employed; but in such a case they might cost more than steel rails; and therefore in the comparison he had made he had taken common iron rails, without extra price, or any guarantee, or extra specification. There had been fifty instances mentioned on the London and North Western Railway, where solid steel rails had lasted six times as long as iron rails; instances of the same kind had been stated on the London, Chatham, and Dover

Railway; and on the North London, in about 40 instances, as far as their experience had extended up to the present time, steel rails had lasted four or five times as long as iron rails. All these steel rails were still in use, and in good condition. As regarded steel-headed rails, he had received a favorable statement from the North British Railway, and that day he had heard from Mr. Ashcroft that some of those rails made for the Swedish Government, referred to in the paper (Plan B, Plate 14), and described in the discussion by Mr. Menelaus, had been laid a few months on the South Eastern Railway, at London Bridge Station. Mr. Ashcroft had given a very favorable report concerning them. However, more experience was needed to prove that they would last three times as long as common iron rails.*

Mr. Bell had expressed a fear as to the supply of pig iron for the future make of Bessemer rails. The same opinion was entertained by the Swedes five or six years ago, when the Bessemer process was introduced. They had actually tried to prove that nothing could be done without Swedish ore. However, very little of the Swedish pig-iron which had come over to this country had been employed for Bessemer steel rails, and there was no doubt that the English hæmatite would be sufficient for some time to come

With regard to the statement that the pig iron made from Norwegian ore at Newcastle contained 0.5 per cent. of phosphorus, Mr. Bell had admitted that the statement applied to ore from a lately opened mine, and that it was not applicable to the great bulk of Norwegian ore. His own opinion was, that if the English hæmatite ore should not prove plentiful enough, the ores from Sweden, Norway, Bilboa, Elba, and America, were sufficient to provide for any future want of pig iron for the Bessemer process.

As to the tests of iron rails, very few railway companies had adopted any more severe than those governments with which he was connected; that had arisen from the severe climate in the Scandinavian countries, the ground being often frozen 3 to 4 ft. deep, and offering in winter little or no elasticity. The speed had to be reduced considerably in the winter, to save both the road and the rolling stock. Although it was well known that greater wear could be obtained from rails containing a large percentage of phosphorus, such as those from the Cleveland district, yet the greater strength, and consequent safety, of the Welsh iron had been taken into consideration, and most of the supply for Scandinavia had been procured from the Welsh districts. There had been no breakages of rails on the Swedish lines, but the Russians had, during the severe frost of 1866, a great number of rails broken; the number amounting to 200 a week during the most severe part of the winter. These Russian rails had been made in this country, nearly of the same section (Vignoles) and weight as the Swedish rails; but the test for the rails destined for Russia was only half as severe as that for the rails for Sweden, the latter being a 15-cwt. ball falling 7 ft. on the rail, supported on bearings 4 ft. apart.

The statement that steel rails in Canada were not injuriously affected by cold so much as iron rails, was what might have been expected. Steel was less subject to the influence of cold than iron, especially when phosphorus was present in the iron. But steel rails had broken when the weather was temperate, in ordinary wear, and even in unloading from the trucks, as had been remarked by several speakers. These rails had been examined, and in the generality of cases they had been found to contain too much carbon. Still there had been several cases in which steel rails had been broken, without being too hard, or containing too much carbon, but from excess of silicon. Formerly it was believed that the silicon was eliminated in the Bessemer process before the carbon was carried off, forming a cinder, which acted on the carbon in the same way as in the puddling furnace. There was now reason to believe that, where there was much silicon present, and very little carbon, the latter disappeared before the silicon, and some silicon was left which rendered the steel brittle. Steel rails had been broken on the Belgian State Railway, and, when they were analyzed, there was found to be no excess of carbon, but so large an amount of silicon as 0.6 per cent. In an-

* Mr. Ashcroft has stated in a letter to the author, dated May 18th, 1869, that trains computed to have weighed upwards of 15,000,000 tons had passed over the steel-headed rails between March, 1868, and May, 1869. These rails were laid in the London Bridge yard of the South Eastern Railway, and he had been surprised to find that they had resisted so perfectly (there being no apparent failure whatever in them), when it was considered that they weighed only 66 lbs. per yard. Opposite to these rails there had been laid some iron rails, weighing 82 lbs. per yard, manufactured by an eminent firm; the latter exhibited evident signs of early failure. It should be noted that all these rails were laid on a curve and on a gradient of 1 in 100, and where the brakes of every train were applied to stop at the platform.

other instance as much silicon as carbon had been found in the metal. These facts had been confirmed by Director P. Tunner, of Vienna, who had communicated the fact to Dr. Percy in a letter, from which he had the privilege of reading the following extract:

"At the iron works at Neuberg, in Styria, the Bessemer metal is carefully assorted after every cast, and is separated into seven series, or different degrees of hardness. The brands to be marked are ascertained on the one hand mechanically, by welding, working, and hardening, by the tensile strength and elasticity; and, on the other hand, chemically, by the Eggertz carburation process; it is only when the same result is obtained by these two methods that the numbers denoting the degree of hardness are marked. A very great difference has been ascertained between the results of the mechanical and those of the chemical methods, in the quality of some Bessemer steel, which was made about eight days ago from pig-iron made by charcoal with $\frac{1}{4}$th its weight of coke. By the first method No. 3 was indicated, that is to say, a very hard steel difficult to weld, but otherwise of good quality. By the other test it proved to be No. 6, that is to say, it had a hardness next to that of wrought iron. In consequence of this very remarkable difference a chemical analysis became necessary, which showed that the steel in question contained only 0.3 per cent. of carbon, but 1.0 per cent. of silicon. It therefore appeared that silicon might occur in steel as a substitute for carbon, and it is particularly remarkable, that it was found in a hard steel. This fact further proves that it is not always true that by the Bessemer process the silicon is first eliminated, and then the carbon. That some proportion of silicon may exist in steel has been long known; and Schafheutl maintains that some silicon is a necessary constituent of steel. But that with 1.0 per cent. of silicon and only 0.3 per cent. of carbon, it is possible to obtain a hard steel, seems to be a new observation worthy of attention."

This confirmed what had been lately found in this country, namely, that the silicon and carbon disappeared simultaneously, and not, as was formerly believed, the silicon first and the carbon afterwards. Therefore, as an excess of silicon had the same influence on the hardness of the steel as an excess of carbon had, and as the amount of the former was more difficult to regulate in the process of steel-converting than the latter, which could be judged of by the flame, he thought it might be concluded that, until further experience was gained, Bessemer rails would require to be as carefully tested as those made from iron. In concluding his remarks, Mr. Sandberg took the opportunity of thanking those gentlemen who had taken part in this discussion, for the valuable information which they had brought forward. That information formed a most useful contribution to the knowledge of the manufacture and wear of rails, and would doubtless tend greatly to the benefit of both the manufacturer and the consumer.

THE DISCHARGE OF RIVERS.*

It is often necessary in the practice of engineering to determine the discharge of rivers, and the velocity and direction of surface and under-currents. In some investigations, also, it is desirable to ascertain the quality of water taken from the various depths, and at different times of tide, so as to know the proportions of sea and fresh water which constitute the mixture and the quantity of solid materials held in mechanical suspension, such as sand or mud.

A few remarks on the mode of conducting these different investigations will form the subject of this chapter.

The most accurate method of measuring water discharge is to construct a gauge-weir, and ascertain the quantity of water flowing over it; but as this process is only applicable to small streams, it does not come within the scope of our subject.

The discharge of a river is generally ascertained by multiplying its mean velocity by its sectional area; and in gauging a river with this object in view, it is necessary first to determine accurately its sectional area in a plane as nearly as possible at right angles to the direction of the current.

* From "The Principles and Practice of Canal and River Engineering," by David Stevenson. Edinburgh: Adam & Charles Black.

This is done by selecting a place where the banks are regular and the stream tranquil. A graduated cord is stretched across, as nearly as possible at right angles to the direction of the current. The depths of the water are carefully taken with a rod, graduated to feet and inches, at distances of 5 or 10 ft. (as indicated by marks on the cord), according to the minuteness of the inquiry to be instituted or the irregularities of the river's bed, and from the data thus obtained an accurate cross-section showing the sectional area of the river can be constructed.

After the observations for the cross-sections have been completed, the measurements for ascertaining the velocity should at once be made before any change in the level of the water, and consequent change in the area, can take place. The velocity with which the water passes over the bed of the river will be found to vary, gradually decreasing from the fair-way or deepest part of the river towards the sides, and from the surface towards the bottom, except in certain exceptional cases, to be afterwards noticed. For the purpose of calculation, therefore, the mean velocity must be determined.

This is most accurately done by determining the surface velocity in the middle of each of the compartments into which the transverse section of the river is divided, by the soundings made, as already explained, and from these surface velocities, by a simple formula, the mean volocity of each of the compartments can be obtained, and the mean of these will be the required mean velocity of the river.

For the purpose of ascertaining the surface velocities, various methods may be employed.

The most common, but by no means the most satisfactory mode of proceeding, is to drop into the water from a boat a float (whose specific gravity is merely great enough to sink it to a level with the surface), at a point about 30 or 40 ft. above the line of section, so as to insure its acquiring the full velocity of the current before it reaches the cord. An observer stationed at the cord notes exactly the moment at which the float passes, and follows it down stream till he reaches the line of two poles, which have been fixed in reference to the observations, when he again notes the exact moment of its transit at the lower station. The elapsed time between the two transits is then noted in the book along with the distance between the two places of observation, which, owing to the irregularity of most rivers with regard to width, depth, and velocity can seldom be got to exceed 100 ft.

This operation has, of course, to be repeated for every compartment of the cross-section.

Dr. Anderson, in measuring the discharge of the Tay at Perth, used an adjustable float, which extended from the surface to near the bottom of the river, and so obtained at once, approximately, the mean velocity of each compartment of the cross-section of the stream.

Certain disadvantages attend the method of measuring velocities by a float, which render it not generally applicable. For example, it is only adapted to rivers of limited breadth, owing to the impossibility of an observer not being able to discover with sufficient accuracy the exact time when the float passes the station lines, if it be viewed from a distance, as, for example, from the bank of a broad river.

There are, however, greater objections than this, which, when pointed out, will be sufficiently obvious to every one. In any part of the river's bed passed over by floats, the slightest irregularity of the bottom produces a disturbance in the motion of the stream and alters its velocity, so that it is not possible, from the time occupied by the passage of the float over the measured distance, to deduce the main velocity at the line of cross-section. It is also impossible, by this method, to obtain a sufficient number of distinct independent observations, applicable to each division of the stream, as the eddies and irregularities of the current which exist in all rivers generally can be passed over by the floats to cross and interfere with each other in such a manner as to destroy all connection between any given series of observations, and the several compartments of the river, whose mean velocity they were intended to ascertain.

The great object is to determine the velocity of each portion of the stream as it passes the line of cross-section; and the best way of doing this is to employ the tachometer* or stream gauge, an instrument of great service in such inquiries.

The current impinging on a vane causes it to revolve, and the number of revolutions made by the vane being registered on an

*The construction of this instrument, and the manner of using it, were described in our May number, page 534.

index which is acted on by a set of toothed wheels, indicates the velocity of current.

Having, by means of the tachometer, determined the surface velocity of the river at each of the divisions of the extended cord, the next step is the reduction of the observed surface to those of mean velocities, which will be readily done by the following rule of Du Buat.

It is not clear that the Du Buat meant this formula to be applied in the manner here described; on the contrary, it rather appears that he meant to deduce from a single surface velocity taken in the centre of the stream, a mean velocity applicable to the whole sectional area. He, as quoted by Prof. Robinson, says, the mean velocity in any open pipe or stream is the arithmetical mean between the velocity in the axis and the velocity at the sides of a pipe or the bottom of an open stream; but it is hardly possible to find a river with a cross-section so symmetrical as to admit of a single central observation proving sufficient, and the formula is therefore often applied to the velocities as measured in the centres of the different compartments into which the river is divided in making the cross-section. Du Buat's rule referred to is as follows:

If unity be taken from the square root of the surface velocity expressed in inches per second, the square of the remainder is the velocity at the bottom, and the mean velocity is the half sum of these two.

Thus let a = the observed surface velocity.
β = the bottom velocity, and
γ = the mean velocity, all in inches per second.

$$\beta = (\sqrt{a}-1)^2 \text{ and } \gamma = \frac{a-\beta}{2}$$

and hence the mean velocity is directly deducible from the surface velocity by the following formula:

$$\gamma = \frac{a + (\sqrt{a}-1)^2}{2}.$$

The mean velocities obtained by calculation are to be multiplied into the area of the spaces, in the centres of which the observations were made, in order to obtain the cubic contents of water discharged in each division; and to obtain the whole discharge it is only necessary to add together the results of the observations made in all the different compartments. The apportioning of the stream into different parts, and treating each as a separate channel, appears to insure a much greater probability of a correct measurement than any method that depends upon assigning to the whole area a common velocity; and it is obvious that this method can be effectually followed only by the use of the tachometer, or by any similar instrument which possesses the advantage of confining its indications to the spot where the sectional area of the river is actually measured. Whenever, as will frequently happen in regular streams, the velocities of several compartments, as ascertained by the stream gauge, are found to be the same, the areas of these compartments may be added into one sum and multiplied by the common velocity.

The velocity of currents in the open sea or in estuaries may also be determined from a boat at anchor, by allowing a float to run out during a given interval of time, and observing the quantity of graduated line which has been let out. Some of the various forms of registering logs are also very suitable for such experiments when the velocities are not below two miles an hour. But as the numerous observations of velocities which I have described always occupy much time, many formulæ have been proposed to shorten the work of calculating the discharge of a stream. I have had opportunities of testing the value of these formulæ by comparing the results they gave with those obtained by the more careful and elaborate process which I have described, and as these generally recognized formulæ are conflicting, and in some cases inaccurate, I shall give the result for the information of the student. Before doing so, however, it is necessary to define certain terms, without which, the application of the different formulæ would not be intelligible. In dealing with the discharge of a river, we are to understand:—

First.—That the slope is the fall on the surface of the water, and is generally expressed in feet per mile, and is ascertained by levels carefully taken.

Second.—That the sectional area is the width multiplied by its average depth as ascertained by means of the section already explained.

Third.—That the hydraulic mean is the quotient given by dividing the sectional area of the channel in square feet by the wetted border or perimeter in lineal feet, also ascertained from the section.

Fourth.—The mean velocity, which may either be deducted from the mean surface velocity by formula, or ascertained directly

by measurement, is that velocity which is used in ascertaining the discharge.

Fifth.—The discharge is the quantity of water yielded by the stream in a given time, and is generally stated in cubic feet per minute, being the mean velocity in feet per minute, multiplied by the sectional area in square feet.

The formulæ which I subjected to trial were:

I. Formula given by Dr. Robinson, founded on Du Buat's investigations:

$$M = \frac{307(\sqrt{d}-0.1)}{\sqrt{S}-\text{Hyp. log. of }\sqrt{S}\cdot+1.6}-0.3(\sqrt{d}-0.1)$$

In which M = mean velocity in inches per second.
d = hydraulic mean depth in inches.
s = the reciprocal of the slope of the surface, or number of feet horizontal for one foot fall.
Hyp. log. = common log. multiplied by 2.3026.

II. Formula given by Sir John Leslie:

$$M = \frac{15}{16}\sqrt{af}$$

In which M = velocity in miles per hour.
a = hydraulic mean depth in feet.
f = the fall on the surface in feet per mile.

III. Formula given by Mr. Ellett for calculating discharge of the Mississippi:

$$V = \frac{8}{10}\sqrt{df}+\frac{df}{20}$$

$$M = 0.8\,v.$$

In which v = surface velocity in feet per second.
d = maximum depth in feet.
f = fall of surface in feet per mile.
M = mean velocity in feet per second.

IV. Formula given in Mr. Beardmore's tables:

$$M = \sqrt{a}.2f \times 55.$$

In which M = velocity in feet per minute.
a = hydraulic mean depth in feet.
f = fall per mile in feet.

V. In addition to these formulæ the writer also subjected to trial the formula of Du Buat as given on preceding page:

$$M = \frac{(\sqrt{v}-1)^2+v}{2}$$

In which M = mean velocity in inches per second.
v = maximum surface velocity in inches per second.

In order to compare these different formulæ, a very favorable situation was selected for ascertaining the discharge of a stream by careful measurements of area and velocity. The result gave a discharge 1,653 cubic ft. per minute. The slope was carefully measured by levelling.

The results were as follows:

	Cu. feet.	
Discharge as measured as above	1653	per minute.
Robinson's formula gave.....	2214	"
Leslie s " "	2474	"
Ellett's " "	2784	"
Beardmore's " "	2335	"
Vth (Du Buat's) "	1950	"

It will be seen from this statement that none of the formulæ afforded a near approximation to the discharge of the small stream to which they were applied. Again, it was ascertained by the late Dr. Anderson, of Perth, after most carefully dividing the cross-section into compartments, and ascertaining the velocity of the stream in each of them, that the discharge of the main branch of the Tay at Perth was 147,391 cubic feet per minute. I ascertained the discharges, as calculated by the different formulæ as above, and the following are the results:

	Cubic feet.	
Discharge per minute by Dr. Anderson.	147,391	per min.
1st. By Robinson's formula.....	153,632	"
2d By Leslie's formula	166,134	"
3d. By Ellett's formula	122,002	"
4th. By formula in Beardmore's tables	156,569	"
5th. By formula assuming the mean deduced from the centre surface velocity as the mean for the whole section.....	179,237	"

The result of these trials, and others which I have had occasion to make, is, that that none of the formulæ that have been proposed will be found generally applicable; but the following may be applied, and will, in most cases, give a pretty near approximation to the velocity and discharge due to a given area and fall, viz.:

$$x = y\sqrt{af}.$$

$$z = \frac{x \times 5280}{60}$$

$$D = s\,z.$$

In which x = the mean velocity of the whole section of the stream in miles per hour.
y = a quotient which is found to vary from 0.65 for small streams, under 2,000 cubic feet per minute, to 0.9 for large rivers such as the Clyde or Tay.
a = hydraulic mean depth in feet.
f = the fall on the surface in feet.
z = the mean velocity of the whole section of the stream in feet per minute.
s = sectional area of stream in feet and
D = the discharge in cubic feet per minute.

It must be kept in view that the application of any known formula to the determination of the mean velocity and discharge of a river is shown, by experimental inquiry, to afford only a rough approximation, unless observations are made embracing the velocities at different parts of the cross sectional area.

In order to render the measurement of discharge useful, care should be taken, when the stream is gauged, to ascertain that it is in a normal condition, by which is meant that it is neither dried to its minimum by a long drought, nor swollen to its maximum by heavy rains. The stream in this normal condition is said to be in its state of ordinary summer water, or at its ordinary summer water level, and to be unaffected by long drought or by heavy rain fall. It is obvious that it is not possible to offer any directions for determining when a stream is in this normal condition, but it will generally be found that the residents on its banks, particularly those engaged in its fishings, if there be any, can tell when the water is at its ordinary summer level.

The fluctuations of a river from its lowest to its highest state are excessively capricious, the amount of flooding which is ascertained to take place in different rivers having no constant ratio either to the summer water which they discharge, or to the area drained by them. This, indeed, does not seem surprising when we consider the very different character, both geologically and agriculturally, of the districts through which rivers flow.

The drainage area in one situation may include large tracts of hill country, having steep and scantily soiled slopes, from which the rain is readily discharged. In another place it may be flat, or gently rising, deep soiled agricultural land, absorbing much of the rain that falls, and giving it off only by slow degrees. Other districts are more or less affected by their geological formation—some strata being less absorbent than others. In others, again, agricultural improvements have an influence on the drainage, sheep-grazing land being less absorbent than arable land.

In rivers which flow from lakes a reservoir is afforded for the storage of surplus waters, which checks the floods below. But, again, as in the case of the Tay, which flows from Loch Tay, a sheet of water, 14 miles long and ⅔ths of a mile wide, it is found that in gales of westerly wind, accompanied by heavy rain, the lake water is heaped up at the outlet, and greatly increases the flood in the river; so that even in the recurrence of floods themselves there are many circumstances which vary their effects, even in the same district. The heaviest floods in all rivers occur with heavy rain and melting snow, for then the bed of the river has, it may be said, to discharge a compound flood, made up of melting snow and falling rain. The construction of railways in India has afforded interesting information as to the floods of the great Indian rivers, which are fully discussed in papers by Lieutenant-Colonel O'Connel and Mr. Howden. The consideration of the data thus obtained has suggested various formulæ for calculating the discharge due to a given area, but the information as to the amount of flood water said to have been discharged from different districts of country is so discordant, that it seems to me to be impossible, with elements so variable, to be found any formula that can be generally useful. The quantity passing off during high floods is variously stated by different authorities from 1 ft. to 30 cubic ft. per minute per acre, according to the district in which the observations were made. But the highest gauging I have ever got was 15 cubic ft. per acre, from a town district of 630 acres, after three days of nearly continuous rainfall. Thunder-storms discharge a very much greater amount during their short duration. It is stated that in August, 1846, during a thunder-storm, 3.3 in. fell in 2 hours and 20 minutes, being 85 cubic ft. per minute per acre.

Perhaps the only general result to be gathered from the published observations relative to floods, is that the flood discharge has a higher ratio to the ordinary discharge in small than in large rivers. This is due very much to the fact that in a small river, a rainfall affects every one of its feeders, whereas in a large river the influence of the rain is limited to one portion of the district only. If, for example, such a river as the Mississippi were subjected to an increase of its bulk similar to that of small rivers, the country through which it flows would be entirely devastated. The safety of such a country is due to the important fact that excessive falls of rain, like hurricanes of wind, while at the height of their fury, are not wide-spread, but act on a comparatively limited portion of the earth's surface. Though we cannot, therefore, deduce from

data so arbitrary any law applicable to rivers in all districts, we are not precluded from dealing with the different sizes of floods discharged from any particular district, and as it is sometimes desirable to ascertain by gauging the average summer flow of a stream, I give the following mode of computing the discharge exclusive of floods, which has been proposed by Mr. Leslie. First the gaugings are all to be set down in a table in the order of their quantities, beginning at the smallest and going on to the largest, or vice versa. The whole number of observations is then to be divided as nearly as possible into four equal parts; whereof the lowest fourth is held to comprehend the extreme droughts, and the highest floods. The average of the middle half is to be ascertained, and all above that quantity is held to be flood-water. A new table is then to be constructed, in which all the gaugings not exceeding the average of the middle half are put down as equal to that average quantity. The average of the whole of the new table is to be considered as being a fair estimate of the water flowing in the stream exclusive of floods.

UNDER-CURRENTS.

I must offer the further caution, that those rules from which the mean velocity is deduced, on the assumption that it bears a constant ratio to the surface velocity, do not apply in many situations which are within the influence of the tide. In surveying the Dee at Aberdeen in 1812, for example, Mr. Robert Stephenson found that while there was an outward upper-current of fresh water, there was an inward under-current of salt water; so that, although the upper stratum was constantly running towards the sea, there was a regular rise and fall of the surface, produced by the tidal waters below. Another instance of such an under-current, though not occasioned by the presence of a river, was found to exist in a marked degree at the Cromarty Firth, where Mr. Alan Stevenson in 1837 found currents greatly exceeding the surface velocity.

It is essential in some inquiries to ascertain to what depth the currents penetrate, and whether under-currents exhibit the same phenomena in regard to direction and velocity as those of the surface; and as these inquiries are interesting and important, and have lately been much discussed in connection with deep-sea researches, they are worthy of detailed notice.

For small depths the tachometer of Woltmann is a convenient and accurate instrument for measuring under-currents. I never used it myself for depths exceeding a few feet, but I understand, from Professor Gordon, that it has been employed in Germany for measuring velocities at a great depth, by using an apparatus erected on a platform, supported on two boats, and that Rancourt used it to measure the velocity of the Neva at St. Petersburg, at depths of 60 ft.; Defontaine, the Rhine, at upwards of 40 ft.; and Funk, many rivers, at depths of from 40 to 60 ft. But as its application under such circumstances may be regarded rather as a purely scientific than as an engineering experiment, it is not necessary to describe it in this place. The direction of the under-currents, which it is sometimes interesting to know, cannot be obtained by means of the tachometer, and I shall describe a plan for obtaining an approximation to both the velocity and direction of the under-current, which was devised and used, I believe, at the Cromarty Firth, in 1837 by the late Mr. Alan Stevenson, when he detected the tidal anomalies already alluded to. It may be well to explain that the waters of the Cromarty Firth pass to and from the sea through the narrow gorge between the Suters of Cromarty, where the width is about 4,500 and the depth about 150 ft. The mean velocity due to the column of water passing this gorge, as deduced from the observed surface velocity, was not sufficient to account for the quantity of water actually passed during each tide, as determined by measuring the cubical capacity of the basin of the firth. This led to the observation of the under-currents through the gorge by means of submerged floats, and it was found that during flood-tides the surface velocity was 1.8 mile per hour; while at the depth of 50 ft. the velocity was not less than 4 miles per hour, being an increase of 2.2 miles per hour. During ebb-tide the surface velocity was 2.7 miles per hour, and at 50 ft. it was not less than 4.5 miles per hour, being an increase of 1.8 mile per hour. The instrument by which these velocities were measured consisted of a flat plate of sheet-iron measuring 12 by 18 in., having a vane made of the same material, and measuring 4 ft. in length, fixed at right angles to the centre of it. The lower edges of the plate and vane were loaded with bars of iron, for the purpose of causing the instrument to sink to the requisite depth; and

it was so slung by the cords suspending it as to preserve the surface of the plate in a vertical plane. This apparatus was secured by a cord of sufficient length to sink it to the required depth, and the whole was attached to a tin buoy, which floated on the surface, its form being such as to produce little resistance to its passage through the water. The buoy served not only to preserve the vane plate at the same depth, but it also indicated its progress through the water in a very satisfactory and often interesting manner. The plate, sunk at the depth of 50 ft., when acted upon by the force of a strong under-current, was hurried along, carrying the buoy, which floated on the surface, along with it, as shown by the buoy passing the floats thrown out on the water as gauges of the velocity of the upper-current. The only precaution to be observed in making such observations, is to exclude that part of the commencement of the buoy's course, which is more rapid than it had ought to be, owing to the effort made by it to overtake the plate, which, being sunk first, has been influenced by the velocity of the under-current before the buoy has been launched.

It is evident, that by means of this simple apparatus we can approximate to the direction as well as to the velocity of under-currents; but it must be kept in view that there are several deranging influences in operation, which tend to render the results obtained merely approximations to the truth.

Since I first described these Cromarty Firth observations in 1842, many efforts have been made to ascertain the existence and strength of under-currents. Messrs. Carpenter and Jeffreys, in 1870, when engaged in their deep-sea researches in the Porcupine surveying ship, endeavored to ascertain the state of the under-currents at the Straits of Gibraltar. The apparatus adopted for by them for this purpose was arranged by Captain Calver, and was identical in principle with that employed at Cromarty Firth; the only difference being that the under-current float was composed of a basket, with pieces of sail-cloth fixed to it, and so disposed as to catch the current. The float was weighted with lead, and the cord by which it was suspended, instead of being attached to a float as at Cromarty Firth, was fixed to a boat, the drifting of which indicated the force and direction of the under-current. It does not appear that more than two or three observations were made with this instrument.

Captain Spratt, who has made several observations on the under-currents of the Sea of Marmora and the Dardanelles, in a paper on the under-current theory of the ocean, in the Proceedings of the Royal Society, states the following as the plan he adopted: "I never attempted such experiments by the use of any bulky object, such as a boat, that offered great resistance to the surface current. I felt, too, that a fixed object as a point of reference, was always unnecessary, such as a buoy or a float attached to a sinker, actually on the bottom. Such observations for testing ocean currents should only be made in connection with a fixed object attached to the bottom, whether in 2,000 or 20 fathoms. The float which Captain Spratt, after experience, found to answer best, was one of thin copper or block tin, suspended like a kite, his observations being in this respect the same as those at Cromarty Firth, in order to obtain the relative speeds of the surface and under-current floats, fulfilled the object of his buoy moored with a sinker. Mr. Henry Mitchell, of the United States Coast Survey, describes an instrument used by him for that purpose. It consisted of a tin cylinder, a few inches in diameter, and long enough to reach from the surface nearly to the bottom. Tubes 40 ft. in length were used for this purpose. They were 3 in. in diameter, made in separate sections, air-tight, but with stopcocks for letting in the water, that they might be practically filled so as to sink it to the proper depth. As the tube drifted nearly upright in the water, with its top protruding a few inches above the surface, its velocity indicated the mean motion of the stream. If it leaned backwards or forwards, it showed that its foot rested on a stratum that had greater or less motion than the surface drift; and if its angle of direction differed from that of the surface log, the action of an under-current was recognized, whose course was at variance with that of the surface drift. Mr. Mitchell also says, that very good results have been obtained by using two hollow copper globes of 2 ft. diameter each, connected by $\frac{1}{8}$th in. wire rope. The sinking globe is filled with water, but the other is loaded only enough to sink it nearly to its pole. The upper globe has a long line secured to it, and its motion is recorded at the same time that an observation is

made with the surface log, like the compound float used by Dr. Anderson at the Tay. Mr. Mitchell says, let us suppose that the two globes present equal effective areas (great circles) to the drifts in which they swim, then their velocity will be a true mean of the rates of the surface and under-currents, *i. e.*, $\frac{1}{2}(x+y)$ where x and y represent respectively these rates; the velocity of the under-current may therefore be found by subtracting the surface rate from twice that of the connected globes." This formula no doubt gives the mean for the velocities of the two strata in which the balls are floating; and it would give the mean for the whole column of water, provided there is a regular gradation between these two observed velocities; but it does not provide for any inequality of velocity, or for any anomalous velocity, such as has been stated to exist at the Dee and at the Cromarty Firth. This objection might, perhaps, to some extent, be removed if it were practicable to suspend balls, similar to those used by Mr. Mitchell, at short intervals on the wire rope; but for engineering purposes, the object of ascertaining the under-currents has, in my experience, always been to calculate the discharge; and it is obvious that for this purpose we must determine the thickness of the different strata moving at different velocities, so as to ascertain the different sectional areas to which the velocities apply, and this is not at one but at several points on the cross section of the channel or passage through which the current was flowing. Until we have some method of ascertaining the velocities at different depths, and the sectional areas corresponding to these velocities, it is not possible to arrive at the discharge, and all observations on the strength and duration of under-currents must be regarded by the engineer, in making calculations, to be simply approximate. The remarkable under-currents are mainly, if not altogether, due, I believe, to the configuration of the bottom, and the circumstances under which the tidal wave approaches and recedes from the shore. A powerful oceanic under-current during flood-tide in a stratum of water of highly specific gravity and low temperature, setting dead along the coast, would naturally creep along the rising bottom of the sea, and flow into the deep inlet of the Firth, mingling imperfectly with the surrounding water, maintaining its character of a distinct stream, and increasing the under-velocity of the flood-tide; and if we suppose a similar rapid counter-current to sweep along the coast at ebb-tide, its tendency would be to draw off the lower stratum of denser and colder water, and thus to increase the velocity at or near the bottom during ebb-tides. Of the existence of such distinct ocean currents, some at great depths, and others superficial, mantaining their character, and mingling slowly with the surrounding ocean, there are many striking examples; among others, the surface current of the Gulf Stream, which, flowing from the Gulf of Mexico, skirts the coast of the United States, and can be traced as a distinct body of matter by its difference of temperature as far as the Banks of New Foundland. In the month of July I found the temperature of the sea, as tested at various points between the shore of America and the edge of the Gulf stream, to average 60 deg. Fahr., while in latitude 41 deg. N., longitude 61 deg. 52 min. W., the vessel being in the track of the Gulf stream, the temperature of the water was 70 deg. After leaving the influence of the Gulf Stream the temperature within a few hours' sail fell to 60 deg., which was the average of the observations made during the remainder of the voyage to the English Channel, ascertained as accurately as the facilities granted to a passenger by a packet-ship permitted. The cause of ocean currents is obscure; they no doubt are occasionally caused or increased by gales of wind; but no current can be generated without a difference of head, which again may be produced either by a difference of level in water of the same density, or by a difference of specific gravity in columns of water of the same height. The examples I have given of the differences of level existing in rivers and estuaries at certain states of the tides, afford sufficient proof of the existence of currents from that cause. It is not unusual to apply the expression indraught to describe the flow of water into a bay or creek, and to hear it used so as almost to imply the existence of some inherent attraction in the bay or creek for the water which flows into it. But the flow of water in all such cases is caused by the pressure due to the difference of level or density, or both combined; and when the bay or creek gets filled up, and its surface attains a sufficient height to balance the pressure of the source of its supply, or

the momentum of the moving column of water where converging shores cause the level of the water to rise, the indraught disappears.

REMARKS ON ROLLING IN A SEAWAY.

From "The Annual of the Royal School of Naval Architecture and Marine Engineering."

One of the most interesting papers in the Report of the Committee on Designs for Ships of War recently presented to Parliament is that by Professor Rankine, entitled "Remarks on the Stability of Mastless Ships of Low Freeboard as affected by the Waves." All the conclusions of the Committee in connection with this subject are based upon the results obtained by Professor Rankine, and endorsed by the Scientific Sub-Committee, so that a few remarks respecting the method of investigation will not be without value. In order to facilitate reference, a reprint of Professor Rankine's paper is appended.

The formula upon which the calculations are based was obtained by Mr. Froude, and is as follows:—

$$\frac{-\theta}{\theta - \theta} = 1 - \frac{T^2}{t^2}$$

in which

θ = steepest slope of the wave.
θ = the maximum angle of the ship's roll relatively to the horizon.
T = wave period.
t = natural period of the ship.

This equation is derived from the following assumptions:—

1. That the waves are all equal and of a uniform period.

2. That the waves are all of a definite trochoidal form.

3. That the reaction of the water pressure on the ship is always perpendicular to the surface of the wave at the part occupied by the ship.

4. That the time of oscillation of the ship, in smooth water, is the same for all angles.

None of these assumptions can be regarded as accurately true, and one or two of them are plainly very rough approximations to the truth. For this reason I have always regarded Mr. Froude's formula as exhibiting the general aspects only of the rolling of ships in waves, and not as affording any trustworthy means of calculating the extent of the roll in any practical case.

The assumption that the time of oscillation of the ship is the same for all angles is alone sufficient to make it necessary to regard this formula with considerable reserve, when it is applied, as in the case under notice, to calculate the rolling of actual ships; because their times of oscillation are far from being the same for all angles, a fact upon which considerable stress is laid in the Report of the Committee.

Professor Rankine's investigation, therefore, applies Mr. Froude's formula to a case of which the conditions differ greatly from the assumptions upon which the formula rests. Apart from this fundamental objection, it appears that in conducting the investigation several additional assumptions have been made which must be regarded with distrust. For example, it is assumed that the wave observed by Scoresby in a passage across the Atlantic is a fair representative of the most dangerous waves to which ships will be exposed; but this is by no means certain. Again, it is assumed that the metacentric period for ships will vary with the amplitude of the oscillations, in the same manner as in a common pendulum, the total range in the stability of the ship being compared with an amplitude of 180 deg. in the pendulum. No mention is, however, made of the similar variation that may take place in the time of the waves, although such variation would seriously affect the investigation. Moreover, in a new design, the position of the centre of gravity and the length of the radius of gyration can only be estimated, and consequently the approximation to the metacentric period may be liable to considerable error, the effect of which is important. As an example of this, it may be stated that if the period for the Devastation had been taken as 11 seconds instead of 14 seconds in the application of Professor Rankine's method, the maximum roll of the ship would have been found 5 deg. greater than that given in the Report. In view of these assumptions, and the errors possibly arising therefrom, the results of the Committee's investigation cannot be regarded as thoroughly trustworthy. These results are practically summed up in the Report by the

statement that 39 deg. is a sufficient range of stability for large sea-going iron-clads to make them safe when rolling among waves, supposing them to be without sail.

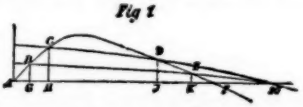
When we pass from the case of a mastless ship to that of a sailing ship rolling among waves, a still more difficult problem is encountered.

One method of investigation that has been suggested may be briefly described as follows: Assuming that a definite angle has been determined for the vessel when rolling without sail among waves, it is proposed to divide the curve of stability into two parts, one being assigned to resist the action of waves, and the other to provide for the effect of the wind on the sails. Supposing Fig. 1 to represent the curve of stability by the line A B C D E F, and C D to be a "wind-curve," assumed to vary as the square of the cosine of the inclination, the curve C D is so placed on the curve of stability as to cover a range equal to that which would be considered sufficient if the vessel had no sail. The part thus cut off from the top of the curve of stability is considered to provide a margin against the rolling produced by the heave of the sea; A C is devoted to the wind, and half of it, A B, is taken as the greatest steady heel under sail consistent with safety.

It appears to me that the details of this method rest upon no valid foundation. First of all, one finds a definite angle selected as the necessary range of stability for the ship, considered without sail; and this has been shown to be a question yet remaining without satisfactory solution. But even accepting this angle as fixed, the mode in which the combined effect of wind and waves is estimated is open to important objections.

Referring to Fig. 1, and remembering

Fig 1



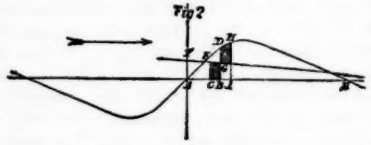
that the interval between the curve of stability and the wind-curve C D is devoted to resisting rolling produced by the waves alone, it will be seen that the front part of the curve from A to C is left to resist the action of the wind on the sails. Hence if, in all cases, the wind came on very gradually, the ship might be safely sailed with a permanent heel represented by A H. But as the wind may come on suddenly, as a gust, A C is divided into two equal parts at B, and A G is taken as the greatest steady heel for safe sailing. If the ship were upright, and at rest, when a wind suddenly acted upon her (the pressure of which would be sufficient to hold her steadily inclined at G), and if it were to continue constant after its sudden application, it would incline the ship as far as H. The supposition is that she would then be safe, because the roll caused by the heave of the sea has already been assumed to be amply provided against by the part of the curve of stability between C and D.

On consideration it will be evident that in thus dividing the curve, a greater range is actually devoted to providing against the heave of the sea than is intended. To absorb the effect of a gust, the front part of the curve from A to H is made double what is required for the steady heel under sail; but by placing the curve C D as described, it is tacitly assumed that the double power of the wind must be provided for at the other end of the curve also, and for this no reason appears. If B E is the wind-curve belonging to the greatest safe steady heel, then the curve from C to E will be the portion of the curve of stability really devoted to rolling in waves with the intensity represented by the wind-curve B E. If this wind were blowing steadily there would be the portion B E of the curve of stability to provide against rolling in waves. And if A C is sufficient to provide for the gust, then, as is said above, C E, not C D, is available to resist rolling in waves.

This objection is, it will be observed, made upon the assumption that the principle of thus subdividing the curve of stability is admitted; but it does not appear that any justification can be found for such division, and this of course is a radical objection to the whole method. The two actions, of the sea and the wind, must generally go on together, and extend over the same portions of the curve of stability, so that it appears improper to devote one portion to the gusts of wind and another to the heave of the sea.

It would, I think, be more nearly consistent with the conditions of a ship under sail and rolling in waves to suppose her to be rolling in the worst waves she is likely to meet, and to suppose her sails to be struck by a squall at that part of her roll at which the gust would be most dangerous.

For instance, suppose Fig. 2 to show a



curve of stability of a ship, which may be assumed to be rolling to leeward under the influence of the waves alone, and with such an oscillation that the roll would end at B D. When she arrives at E a gust may be supposed to strike her, which is represented by the wind-curve F G K, and so the ship is made to roll beyond the position B D to another position, J H, such that the area G H is equal to the area C G. It is evident that the greater the area C G the greater will be the additional angle of roll due to the action of the wind. It is also evident that the area of C G is greater the earlier in the roll to leeward when the wind begins to act. Hence the worst point of the roll at which the wind can begin to act is when the ship has reached the limit of the roll to windward.

This case is represented in Fig. 3. A B

is the extreme windward position of the ship when rolling, which roll would, without any wind, end at C D to leeward. But at A B the wind begins to blow with an intensity represented by the wind-curve A F, and the roll is thus increased, ending at G H instead of C D. The positition of G H is fixed by the condition that the area D E I G shall equal the area A C, and the ship would be safe so long as the area D E F G was greater than the area A C.

Another imaginable case is that where a ship is rolling among waves with a steady pressure of wind, the power of which becomes suddenly doubled at the worst part of the roll. This case is represented in Fig. 4, where K B is the wind-curve for the steady pressure, J K and H C are the extremes of the roll before the power of the wind is increased, and J G E is the wind-curve for the suddenly increased intensity which is assumed to take place when the ship is in the position J K. The gust will cause the roll to be lengthened, say, to the position D F, the area H G E F being equal

to the area J K C G; and in this case the ship will be safe so long as the area H G L F is greater than the area J K C G.

This mode of regarding the subject is by no means free from objection, but I consider it preferable to that previously described, and as more nearly representing the kind of action that must go on when a ship under sail is rolling in the trough of the sea.

One objection to both methods appears on the surface, viz.—that while the curve of stability has reference to the inclination of the ship to the wave-surface, the wind-curve has relation to the inclination of the ship to the horizon. In the foregoing case, therefore, the effect of the wind has probably been reckoned greater than it should be, since it has been taken in proportion to the change of inclination to the surface of the wave, instead of in proportion to the change of inclination to the horizon.

At present I do not see a way to overcome these objections and difficulties, nor should I regard either method of investigation as affording trustworthy means of calculating the precise amount of stability which would be required for new designs. The only safe guide in this matter is, in my opinion, found in experience with successful ships, and in designing new vessels it appears desirable to provide that amount and range of stability which have been proved sufficient in ships that have been thoroughly tried.

The total length of the lines of railroad owned and leased in Pennsylvania by the Pennsylvania Company, is 600 miles. The earnings last year were $22,262,010.08. and the net profits $6,896,403.51, which, after paying interest on indebtedness, is a profit of nearly 12 per cent. on the capital. The Pennsylvania Company lease or own a controlling interest in roads in other States, aggregating a length of 3,200 miles.

LOADS ON GIRDERS.

By E. SHERMAN GOULD, C. E.

Written for Van Nostrand's Magazine.

It would perhaps be difficult to find a better illustration of the difference between pure and applied mechanics, than that afforded by the rise and progress of the attempt to calculate rigidly the strains to which the different members of a trussed girder may be exposed. We say "attempt" because, though many volumes have been written upon the subject, it is quite evident that scientific writers have not yet arrived at that unanimity of teaching which alone certifies that actual truth has been reached.

If 50 years ago the drawing of a trussed girder of a given weight and destined to support a given load, had been submitted to an accomplished mathematician, he would have been astonished if any one had asserted that there was any particular difficulty in calculating the strain on its different parts. He would have at once said—the greatest possible strain must certainly occur when the girder is entirely covered with its load. He would have assumed it so covered and rapidly knocked off the calculation while whistling *di tanti palpati*, or some other opera air. If necessary, he would have demonstrated his results by the integral calculus.

At the present time, such calculations seem to be matters between every engineer and his own conscience. Rigorous mathematical principles are made to prove totally different theories, and it is only since the appearance of Mr. Stoney's works that we may be said to have got even upon the right path of investigation, to say nothing of having reached ultimate results. Fortunately, bridge building has in the meantime proceeded as if we actually knew all about it.

It must be at least admitted that, in the matter of plate girders, or girders with a continuous web, we have nothing further to learn. The method of moments, or to speak non-technically, leverage, enables us to calculate unerringly the strain upon the flanges, while the sheering strain through the web is reached with equal ease and certainty. With the trussed girder, the case is different, and it is around the trussed girder that the controversy between writers upon the subject of bridge strains is now waging.

Before making the following remarks, we will premise that the object sought is to bring the trussed girder as nearly as possible to the condition of the "wonderful one-horse shay," previous to the final catastrophe, of course. That is to say, to so proportion its parts to the ascertained strain upon them that the structure shall be equally strong throughout, and thus be nowhere encumbered with redundant weight.

As investigation progressed, it was discovered that though the flange strain for all girders, plate or trussed, is maximum when the girder is entirely covered with the rolling load, the web strain is not so, but is maximum for any point of the web when the shorter segment of the girder is unloaded. The explanation of this apparent paradox will be shortly given.

Now for the trussed girder. In the first place, it should be understood that the weight on a truss is borne in a manner quite different from a plain beam or plate girder. In the latter case the weight is applied continuously to the flange, and transmitted in all directions through the web by paths which it would be impossible to follow or trace. The strain on any section of the flange or web differs from that on any other section, however close they may be to each other. In the trussed girder the case is different. The weights are applied only to certain points of the flanges, namely, the apexes of the trussing, and are transmitted through the members of the truss by a clearly indicated path.

The strain in every tie, brace, counter-brace, and panel length of flange is therefore uniform through the entire length of such member, and only varies from panel to panel. The calculation by moments, of which the ordinary formula is $\frac{W}{2DL}(Lx - x^2)$ does not therefore apply to trussed girders, nor does any calculation by moments apply to such girders except to simple systems of trussing, like the Howe or Warren, and then only by calculating the moments of each loaded apex separately around the centre of the panel, the flange of which it is desired to calculate. To the compound truss the system does not apply at all.

The true system of calculation is that by

the resolution of forces. Each panel is an independent frame, kept in position by leaning or drawing against its neighbors, and sustaining not only the weight which it bears directly, but also that transmitted to it through and by the others. The whole weight of truss and loading is sustained by the ties and braces, the flanges serving only to keep them in their proper position. In fact it will be readily seen that the vertical or sustaining strain comes entirely upon these members, while the flanges take only the horizontal strain. It would seem as if all difficulty in calculating the strains upon the different members of a truss must vanish when the structure is considered thus, and so it would if certain mischievous axioms had not been generally admitted and reasoned from. We refer mainly to two, viz.:

That balanced weights (*i. e.*, the weights on an uniformly loaded truss) are transmitted direct and totally to the nearest abutment, and that when a truss is loaded with balanced weights, an unbalanced weight acts as if the truss bore no other weight at all. We call these erroneous axioms mischievous because they, particularly the first, have a seeming truth.

In fact, when a truss is uniformly loaded, the balanced weights act precisely as they would were they indeed transmitted undiminished to the nearest abutment. But in point of fact, each weight of a balanced series transmits proportional parts of its whole weight to the two abutments. To generalize, we would state that any weight upon a beam or truss transmits proportional parts of its whole weight to the two abutments. An admission of this fact clears away all difficulty in the calculation of bridge strains. We will not speak of the second false axiom, because a true understanding of the first renders this unnecessary.

We will now state the true axioms upon which the calculation of a trussed girder rests.

1. The weight of the truss and load acts upon the flanges only at the apexes of the trussing.

2. All weights are transmitted in proportional parts to the two abutments.

3. The actual strain on any web member is the algebraic sum of the compressive and tensile strains upon that member.

The strains on the flanges alone are maximum when all the apexes are loaded; the strain on any tie being maximum when it and the apexes between it and the farthest abutment only are loaded.

We see why this is so from the second and third axioms. The weights on the shorter segment send proportional parts through the longer segment, and these act compressively upon the tie. Remove these weights and their substractive influence is no more felt upon the tensile strain, which becomes thereby augmented. In fact, the neatest way to obtain the maximum strain upon a given tie, is to add the weight which the apexes of the shorter segment send to the farther abutment, to the weight borne by the tie when all the apexes are loaded. This method requires less figuring than any other, because, in order to calculate the flange strains, it is necessary first to obtain the strain on the ties when the truss is uniformly loaded, and the maximum strains on these members may thus be readily got by addition.

In a simple truss, such as the Howe, we cannot consider the apexes of the shorter segment as wholly unloaded. There is always half a panel weight of the rolling load upon the tie next nearest the nearest abutment from the tie considered as the last one in the loaded segment. This half panel weight must be taken account of, as it lessens the maximum strain on any given tie by the amount of weight which it sends to the further abutment. In compound trusses this is not the case, as the half panel weight is borne by the tie of another system of trussing.

As a general rule, if a truss be counterbraced throughout, the counterbraces need only be calculated to sustain one panel weight each of the rolling load, as this will be more than sufficient to resist whatever sheering strain may arise from the passage of the rolling load. The greatest amount of sheering strain is brought on the counters when half the truss is covered with the rolling load. In calculating the actual strain (see axiom 3) upon the counters from the rolling load when thus distributed, care must be taken to deduct the strain sent by the apexes between the strained counters and the centre of the truss, to the nearest abutment, as well as the strain from permanent truss load. This will in almost all cases reduce the sheering strain below that which the counters sustain in stiffening the truss, namely, one panel weight of the rolling load, each.

It would seem now, as if the calculating

of a truss were a very simple matter, but an article from "The Engineer," in the July number of this magazine, calls attention to the fact that it is not sufficiently accurate in long trusses to consider the permanent bridge load as a uniform weight per apex, since the panel bridge weight at the centre very greatly exceeds that at the ends. The extra weights of the end ties and braces would go somewhat to reduce this difference, but not to sensibly equalize it, and the difference is unfortunately on the side of danger. This new and perfectly true observation still further complicates the "wonderful one-horse shay" system, as applied to trusses.

THE VELOCITY OF PISTONS IN CRANK ENGINES.

By R. WERNER.

Translation from "Zeitschrift des Vereines Deutscher Ingenieure."

The mean velocity, the mean effective steam pressure upon the square unit, and the sectional area of the piston are the three factors of the effect or work of an engine as shown by the indicator.

Suppose the boiler pressure and the size of piston given, then the determination of the most advantageous velocity depends on one or more of several conditions imposed in each particular case.

First.—If the mechanical effect is to be the greatest possible without reference to expenditure of steam, a maximum will be reached by a fully open throttle valve, giving a velocity that causes unsteady speed and less control. The reason that the power of an engine diminishes for a still higher velocity is this: the effective steam pressure upon the piston diminishes by a greater ratio than that of the increase of velocity. The power product is finally so small that it is equal only to the running of the machine. This velocity is generally so great that it can be practically attained only with danger of bursting the fly-wheel.

Second.—Suppose the least possible expenditure of steam per horse power; then, at first glance, a very slow speed seems most desirable, because this corresponds to the highest possible effective steam pressure. But further consideration shows that in case of less velocity of piston,

(*a*) A certain quantity of steam remains for a longer time in contact with cylinder, piston, and slides, causing loss by condensation.

(*b*) There is further loss due to play of piston and slides.

(*c*) Small velocity would require greater dimensions in parts of machine for the same performance, and there would consequently be greater resistance from friction and greater cost of attendance.

(*d*) Finally, for moderate velocities, and for steam pipes not too small or long, the depression of steam is so small that the ordinary velocities of practice may be increased with advantage. We have proof of this in the case of locomotives. With them great speed is a necessity, in spite of the principle that a "solid" steam engine should go slow to prevent wear and tear, and shocks in joints and bearings, and to insure steadiness.

Wear and tear in general increases with the velocities of sliding parts, and decreases in the ratio of the specific pressure surface. In swift machines the journals, beds, and grooves should be long and made of steel, bronze, or composition.

Steady motion is by no means conditioned upon low velocity. It is rather consequent upon a certain rapidity which depends

(*a*) Upon the ratio of the initial steam pressure to the weight of the parts moving with the piston.

(*b*) Upon the degree of expansion.

(*c*) Upon the length of stroke.

The relation of these three magnitudes up to this time has been determined only for non-crank engines, and especially for pumping engines. In these cases the weight of the piston rod, as an element of construction, is of great import. And the law of motion of the piston and rod are dependent upon this, not being given *a priori*, as in the case of a crank machine, as a consequence of the nearly uniform revolution of the crank. From this simple law of sine or harmonic motion, for any position of the piston may be found the pressure at crosshead and link due to the inertia of the parts moving together with the piston. This pressure at the beginning of the stroke, operates against the active steam pressure, at the middle of the stroke becomes zero, and

varies with the steam pressure till the end of the stroke.

A speed the freest possible from shock, depends upon the condition that the change in pressure (steam *plus* mass pressure) at the link is taken at dead point. This would not be the case if, in consequence of too great velocity, the resistance of masses were initially greater than the steam pressure.

In our further investigations of the limits of velocity, of steady motion of oscillating parts, of steam tension and degree of expansion, we refer to a little work in which the subject is treated with great clearness, reference being made to experiments. The title is, "Ueber Dampfmaschinen mit hoher Kolbengeschwindigkeit." Von Radinger. Wien, 1870.

After referring to the influence of oscillating parts, the author remarks that the pressure or indicator diagram represents the work communicated to the crank in its total area, but not in the sections. He then finds the law of "mass pressure" or resistance upon a crank-pin with infinitely long crank arm as follows:—

Let P = weight in kilograms of oscillating parts.
r = length in metres of crank.
$l = 2r$ = length of stroke.
n = number of revolutions per minute.
$w = \frac{2\pi r n}{60}$ = initial velocity of crank-circle.
f = piston area in square centimetres.
L = length of link-rod.
g = 9.808 metres.

Suppose the motion of the piston horizontal, then the horizontal pressure $\frac{P}{g}$ is equal to the horizontal component of the centrifugal force of an equal mass attached to the crank. This centrifugal force is

$$F = \frac{P w^2}{g r}.$$

Its horizontal component is F cos ω, and the magnitude referred to the unit of piston-area is

$$q = \frac{F}{f} \cos \omega.$$

If s = piston space then

$$r - s = r \cos \omega.$$

hence

$$q = \frac{F}{f} \frac{r-s}{r}$$

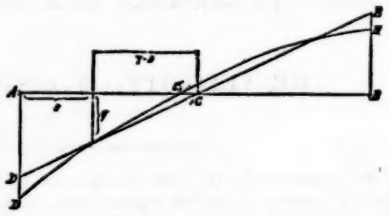
which is the equation of a straight line.

In Fig. 1

$$AD = \frac{F}{f} \text{ and } AC = BC = r.$$

In case of a rod of finite length instead of this straight line, we have a curve E_1 F_1. The areas of the triangles A_1 C_1 D_1 and B_1 C_1 E_1 are numerically equal; but the former is negative, the latter positive.

Fig. 1.



The work due to the "mass-pressure" is represented by the areas of these triangles, just as the work of steam is shown by the indicator diagram. The total work from one dead point to the other is zero; consequently the total area of the corresponding diagram must also be zero.

If the ordinate q, Fig. 1, represents the mass-pressure per square centimetre of piston surface, and p is the ordinate in a steam-diagram on the same base, representing the steam pressure on the same unit. We get by algebraic addition of q and p, the ordinate t of a total-pressure diagram (Fig. 2), representing the work of steam transmitted through the crank-pins.

Fig 2.

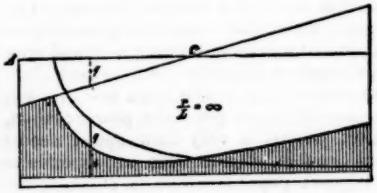
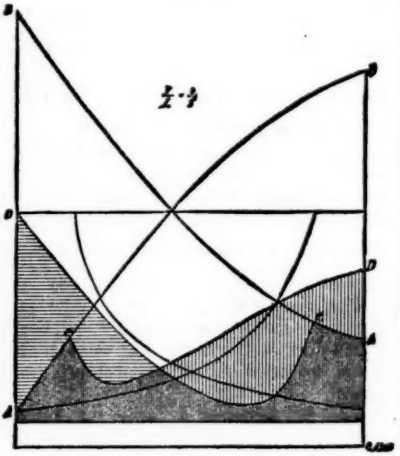


Fig. 3 is a diagram for an engine of $\frac{1}{5.5}$ feed. AB and A_1 B_1 are the limits for the pressure due to acceleration; A C D and A_1 C_1 D_1 are the limits of result and pressure on the crank.

In the case represented in Fig. 4 there is a change of pressure, and a consequent shock upon the crank at B. Either the velocity is too great for the steam tension, or if this velocity is to be maintained, then, to prevent shock, the tension must be increased and the feed must be diminished; the latter, in order that the work of the steam may not

be increased. Hence follows the important conclusion, that the advantage of a high

FIG. 3.



steam tension with corresponding expansion is the same as that of great velocity.

FIG. 4.

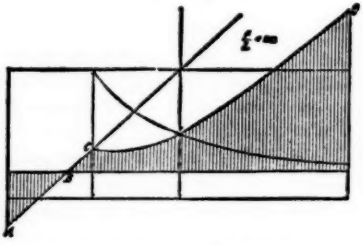


Fig. 5 illustrates a velocity too great, causing change of pressure at C and D.

FIG. 5.

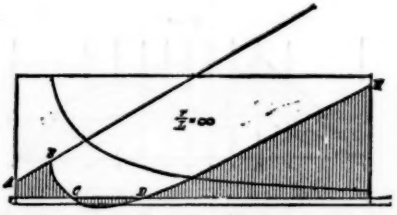
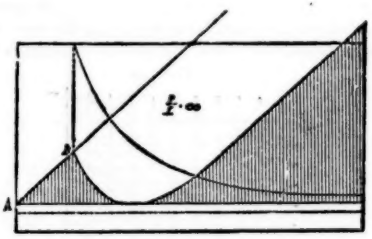


Fig. 6 represents the maximum of velocity and the minimum of supply.

The author gives the following formula

FIG. 6



for the calculation of the maximun velocity of piston :

$$l n^2 = \frac{180\, g}{1+\frac{r}{L}} \cdot \frac{f}{P}(p_1 - p_2)$$

Hence the supply can never be less, with reference to the final pressure, than

$$\frac{l_1}{l} = \frac{p_2}{p_1};$$

or, with reference to the acceleration, than

$$\frac{l_1}{l} = \frac{p_1}{(3\, p_1 - p_2)}$$

In these formulas p_1 is the steam pressure in square centimetre kilograms at the period of full supply, and p_2 is the counter-pressure. Expansion is assumed to follow Mariotte's law.

The author has found the value of $\frac{P}{f}$ seldom less than 0.42 or 0.30, and for safety takes

$\frac{P}{f} = 0.28$ for high pressure engines.

$\frac{P}{f} = 0.33$ for low-pressure engines.

Tables calculated by these formulas give values of initial pressure, stroke, number of revolutions, and minimum of supply corresponding to maximum velocities of the piston.

After determination of the greatest permissible velocities, it is of great importance to determine the limits of velocity consistent with steady speed.

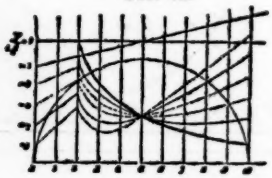
This is accomplished if the pressure perpendicular to the crank changes as little as possible; or if the differences of work in the fly-wheel corresponding are the least possible. The tangential pressure must be found.

Illustration is given in Fig. 7A.

The semi-circumference of the crank-circle is developed into the base line, and the tangential components of horizontal pres-

sure are laid off as ordinates. Since the fly-wheel during half a revolution gives out as much work as it receives, the excess, represented by the areas under the line A B,

FIG. 7A.



must be equal to those lying below. As the direct calculation of that value of $\frac{F}{f}$ for which the excess is a minimum is difficult, the author takes the value for which the limiting curve is horizontal in its mean course, or coincides in direction as nearly as possible with the line of mean resistance. With this assumption we have for greatest steadiness

$$\frac{F}{f} = 2\,p_1 \cdot \frac{l_1}{l}$$

It follows that—

(1.) The velocity of most advangtageous speed increases with the tension and the supply.

(2.) If the maximum steadiness is to be had, the feed must not pass the limit $\frac{l_1}{l} = \frac{1}{2}$, because in this case $\frac{F}{f} = p_1$; the velocity becoming as great as permissible.

(3.) For supply greater than 0.5 there is no velocity corresponding to steady motion; and an engine with full supply will work more steadily as the velocity of piston diminishes. So far as the value $p_1 \frac{l_1}{l}$ expresses the final tension we may say

That the steadiness of an engine is the greatest for that velocity with which there is a pressure at the beginning of each stroke equal to twice the final pressure on the piston.

The following is a table for velocities with greatest steadiness at the highest performance (minimum of supply):

Stroke (metres)....	0.50	0.75	1.00	1.50	2 00	
Non-condensing.	175	143	123	100	87	Revolutions per minute.
	2.9	3.6	4 1	5.0	5.8	Piston path per second.
Condensing.....	65	50	46	38	32	Revolutions per minute.
	1.1	1.32*)	1.53	1.90	2.15	Piston path per second.

This table is calculated according to the following formulas:

For condensing engines

$$\frac{l\,n}{30} = 1.53\sqrt{l}$$

For non-condensing engines

$$\frac{l\,n}{30} = 4.1\sqrt{l}$$

If c = piston-space per minute, and
h = the stroke in feet (Rheinl.);
for high-pressure engines $c = 439\sqrt{h}$
" low " " $c = 164\sqrt{h}$

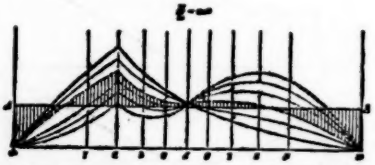
The velocities for infinitely long connecting-rods, corresponding to maximum steadiness, apply without modification to rods of finite length. For, in the last case the velocities up and down are as

$$1+\frac{r}{L} : 1-\frac{r}{L}.$$

The mean between these is the value for an infinitely long rod.

A diagram of tangential pressures for coupled machines is obtained by junction of separate diagrams and addition of coincident co-ordinates. Fig. 7B. shows the

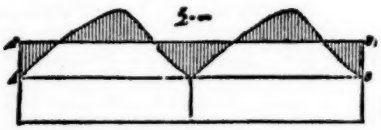
FIG. 7B.



application to a twin-engine with rectangular set. The resultant lines of limit for $\frac{F}{f} = 0$ to 5 give the same line of limit as that of Fig. 8.

The mean line of resistance A B naturally lies twice as high as AB in Fig. 7.

Fig. 8.



If we find that the velocity of a twin-engine is without influence upon the steadiness, still we cannot infer uniformity of motion of the fly-wheel, for this does not always increase in the ratio of the square of the velocity. Motion without shock lies between the velocity limits given above. It follows from the above that in the case of Woolf engines, those in which small and large cylinder each has its separate rod are not as advantageous as those with coupled rods.

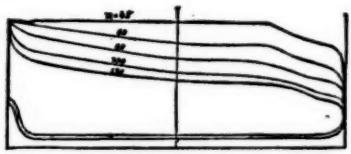
The author next considers the subject of regulation of speed, and shows the superiority of the expansion to the throttle-valve regulator. He concludes that an engine under high velocity is but slightly influenced by the throttle regulator; that there is no decrease of tension in the boiler; and that regulation should depend on the supply. In this case one need not be confined within the ordinary limits of "experience," which depend on different conditions, but may use higher velocities than heretofore employed.

Next is taken up the influence of distribution upon motion of parts. In order to insure steady speed a certain lead and initial opening are necessary. The proper relation of these also causes the quietest motion of parts.

Perfect regulation of distribution of steam is possible only with the help of the indicator-diagram. The fact that the speed of English engines is greater than that of ours is to be attributed to the more frequent use of the diagram. The necessary conditions of steady speed are quick opening and closing of the steam passages by means of sufficient range of slides, proper exhaust port at change of stroke, and wide steam-passages.

The diameter of the steam-passages should be such that the mean velocity of the steam is in all cases 30 metres. It depends both on area and velocity of piston. Indicator experiments on an engine have shown this velocity to be permissible with fully open cut-off; as illustrated in Fig. 9. The diameter of passage is to that of cylin-

Fig. 9.



er as 1 : 15; and for 45 revolutions per minute, and 0 velocity of piston, 2 metres per second; giving 30 m. velocity in the passages, the tension is not materially diminished. But with greater velocity it diminishes considerably.

While the pressures of the steam on the bottom of the cylinder and of the piston on the crank, with reference to the stability of the frame, are counteracted within the engine, the "mass-pressures," as above defined, in horizontal engines act as exterior forces tending to shift the whole system. Although these horizontal oscillations may be counteracted by sufficient foundation, yet, in order that steadiness may be insured to a swift engine, a counterpoise attached to a prolongation of the crank is desirable. This would generate a centrifugal force tending to lift the crank from its place. But this vertical action is not as hurtful as the horizontal. It is advised to take the moment of the counterpoise at 0.5 @ 0.8 of the weight of the rod.

STEAM HAMMER FOUNDATIONS.*

By Mr. THOMAS GILLOTT.

The introduction of the steam hammer in 1843, rendered possible the production of forged masses of iron, greater than had previously been thought of; and since that period the power of this useful agent has been progressively increased to meet the requirements of modern engineering.

As might be expected in the application of new inventions, experience was in a great measure the guide as to what proportions

* A paper read before the Leeds Association of Foremen Engineers.

should be given to the foundations for steam hammers of different sizes; and the various recommendations of some of the best and earliest makers indicated the want of some governing scientific principles, in order to secure that uniformity always apparent where results are certain.

The elaborate investigations of the laws of impact, by Airy, Whewell, and Moseley, have been extensively applied to practice in the case of timber and other driven pile foundations, so that the sustaining powers of these structures are known with certainty; but a discussion of the principles that should guide the engineer in the design of that portion of the steam hammer not supplied by the makers of the hammer proper, has never, to the knowledge of the author, been published.

In ordinary steam hammers a mass of metal, of a certain weight, is raised to a given height, from which it falls—in some cases with the velocity due to gravity alone, and in others this velocity is accelerated by additional forces; the two classes being known as single or double acting, as the case may be. The accumulated work at the moment of impact is expended—first in the compression of the material operated upon to the extent of its own inertia and the resistance supplied by the foundation, and, secondly, in that of the anvil and subjacent foundation. In the case of a defective foundation, the block sinks; and the expense and inconvenience (often involving considerable loss) of packing it up from time to time, are well known to those whose office it is to maintain steam hammers in working condition. It is the object of the investigation which follows, to show how this defect may be avoided; and it is assumed that the condition to be fulfilled by a perfect foundation is, that the anvil and other portions of the hammer shall maintain a constant level when at rest. This assumption also involves another, viz., that a certain level, immediately underneath the foundation proper, sustains no pressure (which pressure is fixed at a certain maximum) that will cause it to yield, and under which it is perfectly rigid.

In the earliest examples the standards and cylinder were quite independent of the block and anvil; and afterwards, partly to maintain all the parts of the hammer in the same relative position, and partly to economize material by opposing the whole mass of the fixed portion to resist the impact of the moving weight, another form was introduced, in which the standards and cylinder were secured to the block; but, notwithstanding its apparent advantages, the effects of repeated blows upon the joints caused this plan to be generally abandoned for large hammers, and the primary one reverted to. The helve, now becoming obsolete, scarcely calls for notice; and the horizontal duplex hammer requiring no foundation to resist the impact of its blow, does not fall under consideration; and the case of the connected block being included in that of the independent one—the difference being that no side walls are required to support the framing and cylinder—the consideration of the principle of the latter will be sufficient.

The rigid level at which the foundation proper commences, being obtained, the portions to be considered are—first, the side walls; second, the bed-plates to which the standards are secured; and, lastly, the block and elastic material interposed between it and the rigid level before mentioned.

Where stone is cheap, it is, no doubt, the most suitable material for the side walls; their office being to support the framing, and, by being secured to the feet of the standards, prevent lateral vibration, it is advisable to secure as much weight and as broad a base as possible; the masonry being carried up from the rigid bottom in order that the shock of the falling hammer may not be transmitted to the various connections so as to injure them. The whole weight should be made available by passing the holding-down bolts through the total depth of the masonry; making the lower ends accessible, so that the bolts can be withdrawn with little trouble when it is necessary to do so. Where stone cannot be readily obtained, Portland cement concrete blocks may be used with great advantage.

The bed-plates form a very important part of a steam hammer foundation; and the tendency of the standards (when double standard hammers are employed) to spread outwards is very great; more especially in those hammers where many side blows have to be given, as in making boiler-plate slabs or shingling, and the standards not bolted together at the bottom of the guides. It is scarcely practicable to cast bed plates for large hammers in one piece, on account of their unwieldly size and the liability of getting broken; but each standard ought to be well bolted to its own plate, and also

wedged in joggles cast on the latter; the plate being extended front and back and the holding-down bolts kept wide apart, in order to give as much lateral steadiness as possible. Where the latter point has not been attended to (and there are numerous instances), the attachment of guy rods to the upper portion of the framing has been found necessary. To connect the two bed plates together, the author prefers two very strong wrought-iron tie rods (one on either side), with T heads, let into corresponding recesses in the plate while hot, and the standards adjusted in the joggles (before mentioned) previous to being wedged. Instances have fallen under his notice in which this form of connection has been subjected to the strains of 10 years' working without visible alteration, while the more modern plan of intermediate plates bolted through flanges cast on the under side, have required renewals, sometimes of bolts and sometimes of plates, in less than one-third of that time; besides having allowed the spreading of the base to a considerable extent.

The anvil-block is the most important part of the foundation, and requires a more scientific investigation than those parts which have been before treated of, in order to determine the necessary amount of material, and avoid undue waste of it. As to the mass that should be placed to resist the impact of a steam hammer, nothing, so far as the author is aware, has been published; therefore, in the following analysis of the conditions which determine it, only such steps of the demonstration are assumed for which reference can be had to known authorities. Moseley ("Mechanics," p. 591) shows that when a body of the weight W_1 moving with a velocity V_1 impinges upon another body of the weight W_1 at rest, at the instant of greatest compression, both will move with a common velocity

$$V = \frac{W_1 V_1}{W_1 + W_2}$$

which is totally independent of the nature of the bodies and their elastic properties; and (p. 597) in the impact of two elastic prisms (whose length is L, section K, and elasticity E), one of which rests against a fixed surface at one extremity, and at its opposite extremity receives the impact, in a horizontal direction, of the other prism, the limits of perfect elasticity not being passed, the maximum driving pressure is represented by

$$P = V \left\{ \left(\frac{L_1}{K_1 E_1} + \frac{L_2}{K_2 E_2} \right) g \right\}^{-\frac{1}{2}} \sqrt{W}$$

In the case of the steam hammer it may be observed that generally the *vis inertia* of the material operated upon may be neglected; since the weight of the smallest forgings that require the whole force of the hammer is small, compared with the hammer itself; when, however, it requires to be taken into account, the maximum working fall should be that from which the velocity is derived and the weight of the smallest forging deducted from the weight of block given by the formula for it. Further, the case of the attached block will be met by considering as "block" the whole mass that receives the impact.

Let W_h = the weight of hammer.
" W_b = weight of block.
" V_h = velocity of impact.
" V = velocity at greatest compression.
" $\frac{L_b}{K_b} = \frac{\text{length of block}}{\text{area}}$.
" $\frac{L_f}{K_f} = \frac{\text{depth of elastic substratum}}{\text{area of base}}$.
" E_b = modulus of elasticity of block.
" E_f = its modulus of elasticity.
" P = the greatest driving pressure the substratum is capable of sustaining in addition to the weight of block, etc.
" g = force of gravity = $32\frac{1}{2}$ feet per second.

Then, atmospheric resistance and friction being neglected,

$$V = \frac{W_h V_h}{W_h + W_b}$$

$$P = \frac{W_h V_h}{W_h + W_b} \left\{ \left(\frac{L_b}{K_b E_b} + \frac{L_f}{K_f E_f} \right) g \right\}^{-\frac{1}{2}} (W_h + W_b)^{\frac{1}{2}}$$

Solving this equation with respect to W_b,

$$W_b \frac{W_h^2 - V_h^2}{P_2} \left\{ \left(\frac{L_b}{K_b E_b} - \frac{L}{K_f E_f} \right) g \right\}^{-1} - W_h$$

which is the equation to the weight of the anvil block when the weight of hammer, velocity of impact, greatest driving pressure, and proportions and elasticities of block and substratum are known.

In the single-acting hammer the velocity of impact

$V_h = \sqrt{H 2g}$; (H being the height of fall;)

but if this velocity is accelerated by a constant force, F, during the fall, the increase will be $V_{hf} = \frac{F g}{W_h}$.

To illustrate the application of the foregoing principles, let the hammer and block be of cast iron; the ratio of the height divided by the transverse section in feet be $\frac{1}{2}$,

and the modulus of elasticity = 7,600 tons for a base of 1 sq. in. Let the elastic substratum be of oak, capable of sustaining a driving pressure, in addition to the weight of the block, etc., of 50 tons per sq. ft.; the modulus of elasticity being 760 tons per sq. in.; the whole being supported by a rigid bed of stone, concrete, or other material, the level of which must not be depressed; then the following table will give the sizes of blocks for single-acting hammers under the conditions stated:

Weight of Hammer.	Height of Fall.	Velocity of Impact.	Area of base of block.	Thickness of Timber under Block.	Weight of Anvil Block.
Tons.	Feet.	Feet per sec.	Square Feet.	Feet.	Tons.
.5	1.5	9.75		1.	1.21
1	2.	11.38	12.	1.25	2 89
1 5	2 5	12.88	20.	1.5	5.65
2.	3.	13 94	25.	1.5	9.67
2 5	3.25	14.5	35.	1.66′	12.38
3.	3.5	15.1	40.	1 66′	15.8
4.	4.	16 1	50.	1.83′	21.89
5.	4 5	17.1	60.	2.	27.86
6.	5.	18.	70.	2 33′	32.6
8.	5.5	18.8	80.	2.5	48.8
10.	6.	19.7	90.	2.66′	71.4
15.	8.	22.76	100.	3.	182.

For double-acting hammers, let steam be admitted on the upper side of the piston, and act with a pressure of 11.2 lbs. per sq. in. for the whole stroke, on an area of 100 sq. in. for every ton weight of the hammer. The velocity will then be increased about 50 per cent. above that of the single-acting hammer; and the blocks for ½, 1, and 1½ ton hammers would require to be 3.35, 7.75, and 14.5 tons respectively. It does not always happen that double action is required for the whole working length of the stroke, therefore the class of work will influence the weight of the block; but when a lighter block is used on this account, more elasticity should be given to the substratum, in case the steam is allowed, by any accident, to act for the full stroke, and thus prevent permanent depression. To take the case of a hammer making boiler plate slabs; the work generally requires the full force of the hammer, while the piece operated upon is comparatively light, and the total fall but slightly diminished; here a block of maximum size is required.

The work accumulated before impact is $\frac{W_h V_h^2}{2g}$; and that expended upon the material operated upon $= \frac{W_h W_b V_h^2}{2g(W_h + W_b)}$; therefore the amount of work lost is equal to the difference

$$= \frac{W_h + W_b}{2g} V^2 = \frac{P}{2}\left\{\frac{L_b}{K_b E_b} + \frac{L_l}{K_l E_l}\right\}$$

Suppose that the weight of the anvil block is fixed, and that the substratum has to be sufficiently elastic to prevent permanent depression; the depth of it would have to be

$$L_l = K_l E_l \left\{\frac{W_h^2 V_h^2}{P^2 g (W_h + W_b)} - \frac{L_b}{K_b E_b}\right\}$$

(The last factor within the brackets may be neglected.)

Applying this to the case where a block weighing 3 tons is used for a 20 cwt. double acting hammer, the pressure of steam, elasticities, etc., being as previously assumed, the oak packing would have to be 3.92 ft. thick, in order that the level may be maintained constant when the hammer is not working. Bearing in mind that all work expended after the period of greatest compression is attained, is lost, the necessity of large anvil blocks, where maximum effect is required, will be easily seen.

The form of the block is, to some extent, governed by local considerations. The simplest plan is, no doubt, to cast it in one piece, but there are cases where it is not practicable to do so; nor is there any objection to it being cast in layers, provided that the joints are made to abut firmly, and secured both vertically and horizontally. In any case it is advisable to place large blocks in joggled base plates, so as to conveniently admit of the lateral adjustment often required after being in work. By making the block high in proportion to its horizontal section, slightly increased elasticity is obtained; but the danger of unsteadiness and deviation from the vertical, through

unequal yielding of the base, is incurred. Increased lateral dimensions may be obtained by making the block hollow, but generally the space is so limited that it is necessary to adopt the solid form to secure the required weight, when the block is independent; although it is sometimes made hollow, and frequently so in some forms of the blocks to which the standards are attached; but a case of a block on this principle breaking through the centre, having fallen under the author's notice, confirms the opinion that the mass to receive the impact ought to be concentrated as much as possible immediately under the hammer.

The general conclusions to which the foregoing reasoning has led, may be briefly stated:

1. That to secure a perfect foundation, the level at which it commences must be perfectly rigid under the working pressure; and, if not naturally, must be made so.

2. That (in all but the smallest tilt hammers) the block should be independent of the standards and cylinder.

3. That the weight of the block should be regulated by the work to be done on it, and the necessary area of base given and quantity of elastic material placed underneath it, to prevent undue pressure on the formation level.

4. That the bed plates for double standard hammers should be well held together (when not formed by a single casting), and all the work of the best description.

But these conclusions, and what has led to them, are submitted more for criticism than as demonstrated laws; with the hope that their being sifted by practical men—such as compose this Association—may elicit something more true, if not absolute truth.

DREDGING ON OCEAN BARS.

By Q. A. GILLMORE, Major of Engineers, Brevet Major-General U. S. Army.

Written for Van Nostrand's Magazine.

A novel device for utilizing the powers of the centrifugal drainage pump, has recently been put in successful operation by the writer, in deepening the channel over the bar at the mouth of the St. John's River, Florida. Upon this bar the ocean swell which constantly prevails, is of such exceptional magnitude and violence, that the usual method of dredging into lighters or scows, ordinarily pursued in still water, is entirely impracticable.

After sundry futile attempts to get the bar channel deepened by contract, the following plan was recommended by the superintending engineer and approved by Gen. Humphreys, the Chief of Engineers, viz.: to provide a suitable steamer by charter and fit her out with a 9-in. centrifugal drainage pump, two branches of 6-in. suction pipe, and timber bins on deck for holding the sand pumped up from the bottom. The pump engine to be driven by steam from the steamer's boiler, and the sand to be discharged overboard, at selected points, by flooding the bins with clear water from the pump.

DESCRIPTION OF THE DREDGING STEAMER AND APPARATUS.

The steamer, originally built for carrying passengers and light freight, is 132 ft. long on the keel, 24½ ft. broad on the beam, and when ballasted to an even keel draws about 5½ ft. of water. She was modelled with a view to speed, and carries only 100 tons on a draft of 7 ft., is strongly built, with side wheels and short guards, has one low-pressure engine of 120 horse power, and ample boiler capacity.

Although the most suitable boat for the purpose that could be chartered at the time, she is not exactly adapted to the work required of her, on account of her comparatively deep draft and small carrying capacity, which rendered it impossible to prosecute work continuously during the periods of low water. A boat with more beam, and a fuller model fore and aft under the water line, would have been better.

The pump, a No. 9 centrifugal drainage pump of the Andrews patent, is located on the main deck aft, about 35 ft. from the stern post. Its suction and discharge openings are each 9 in. in diameter. To the suction opening there are connected by a 2-way branch pipe, two 6-in. suction pipes, instead of one 9 in. as usual, the object being not only to work on both sides of the boat simultaneously, but to render the necessary handling of the pipes as easy and

prompt as possible. There is, on the other hand, considerable disadvantage in operating with two suction pipes instead of one, on account of the greater amount of friction for an equivalent suction capacity; for while a 9-in. pipe has an area of 81 circular inches, two 6-in. pipes have an aggregate area of only 72 circular inches. The frictional surface is therefore increased as 27 to 36, making the disadvantage from or loss by friction from this cause as 2 to 3.

As a partial compensation for this increased amount of friction, an increased velocity is given to the water in suction pipes of less aggregate area than the discharge pipe, and a larger proportion of sand is thereby carried up.

It was necessary also to encounter another disadvantage, by using several bends, of which there were two in each of the suction pipes, and one in the discharge pipe, those in the suction being each one-eighth of a circle, and that in the discharge pipe, of one-fourth a circle. These bends reduce the delivery at the rate of 10 per cent. for each turn of 90 deg., and about 6 per cent. for each turn of 45 deg., the reductions in each case being calculated upon the quantity passing the preceding bend.

Thus, the first one-eighth bend in the suction reduces the quantity to 94 per cent., the second to 88 per cent., and the one-fourth bend in the discharge to 79 per cent. The disadvantages, therefore, under which the apparatus labored, may be briefly summed up as follows:

1. The loss by friction, due to the use of two 6 in., instead of one 9-in. suction pipe, is increased 50 per cent.

2. The unestimated loss by friction, due to the use of suction pipes three times as long as the height to which the material is to be raised.

3. The loss of 21 per cent. by bends in the suction and discharge pipes.

The engine used to drive the pump consists of two cylinders connected upon one crank, at right angles to one another, and 10 in. in diameter by 10 in. stroke, each. Steam is conveyed from the steamer's boiler to the pump engine through a 3-in. iron pipe, the usual pressure carried upon the boiler being about 25 lbs. to the sq. in.

This pressure develops about 26 useful horse power (after deducting 25 per cent. for friction of engines and difference of pressure in the cylinder and boiler), and gives a speed of about 180 revolutions per minute to the engine shaft. On this shaft is a pulley 42 in. in diameter, carrying a rubber belt 12 in. wide, communicating the power to the pump shaft through a pulley 24 in. in diameter, thus giving the pump disc and wings about 315 revolutions per minute. This speed in the No. 9 pump is equal to the work of raising 3,000 galls. of clear water per minute, 30 ft. high, through a 9-in. straight vertical pipe. The actual height raised above the water on the St. John's bar varies with the amount of sand taken on board, from 10 to 11 ft., but as the pipes are 50 ft. long, with bends, and are in two branches instead of one, and as a mixture of sand and water is heavier and more impeded by friction than clear water, the loss by friction from all these causes combined, reduces the useful work of the pump considerably below the average attainable under more favorable conditions. For these reasons, although 200 revolutions of the pump disc per minute will easily raise 3,000 galls. of clear water, 12 ft. high, through a straight vertical 9.-in pipe, 300 revolutions are required to raise 2,500 galls. of sand and water 11 ft. high through the two inclined suction pipes having two turns each, discharged through a pipe having one turn. To prevent the ends of the suction pipes being lifted off the bottom by the pitching of the boat, and, as a precaution against accident, a portion of each pipe is made flexible, being composed of 6-in. rubber hose stretched over a coil of wire. In addition, the ends are loaded with an iron frame or drag, each weighing about 250 lbs., which is intended to move flat along the bottom during the operation of dredging. To the under-surface of this frame, directly below the mouth of the pipe, a number of teeth or knives are attached to stir up the sand and aid its entrance into the pipes. A chain attached to each drag and leading to the deck of the steamer on either side, takes the strain from the pipe when the drag is down and the steamer in motion.

Tackles are arranged for lifting the pipes from the bottom when not dredging, or when pumping clear water to discharge the sand from the bins.

For receiving the sand, bins are located along the main deck, fore and aft, on each side of the steamer's engine, each bin being provided with a sliding gate over the steamer's side, which can be opened and closed at pleasure. The bottom of the bins slopes downwards towards the gates. They are

filled from two open troughs, one from each branch of the discharge pipe, provided at suitable intervals with valves or gates, so that the load can be distributed to the bins wherever desired.

The proportion of sand that can be pumped depends greatly upon its specific gravity and fineness. The calcareous and argillaceous sands flow more freely than the silicious, and fine sands are less liable to choke the the pipe than those that are coarse. When working at high speed, 50 to 55 per cent. of sand can easily be raised through a straight vertical pipe, giving for every 10 cubic yards of material discharged, 5 to 5½ cubic yards of compact sand. With the appliances used on the St. John bar, the proportion of sand seldom exceeded 45 per cent.,

Tabular Statement of work done during a period of Seven Months.

For the month ending	At work on the Bar.		Quantity of Sand removed.		Cubic yds. removed per minute.	Cost per cubic yard.
	No of Days.	Time in hours.	No. of loads.	Cubic yards.		
Nov. 30, 1871.....	19	73½	119	3222	0.734	$0.8476
Dec. 31, 1871.....	13	49½	96½	2542	0.853	1.2195
Jan. 31, 1872.....	18	61 7/12	119	3430	0.928	0.8256
Feb. 29, 1872.....	16	68 1/20	155	4600	1.127	0.5915
March 31, 1872....	20	87 3/20	198½	5545	1.059	0.5591
April 30, 1872.....	17	103½	196	5745	0.922	0.3977
May 31, 1872.....	25	179 1/12	356½	11885	1.105	0.2608
Totals.....	128	622 17/20	1240½	36969		

generally ranging from 30 to 35 per cent. when working under the most favorable conditions.

In pumping 2,500 galls. or 12.6 cubic yards, of sand and water per minute, we would therefore get from 3.7 to 4.3 cubic yards of sand. During the early stages of the work, before the teeth under the drag had been properly arranged to aid the flow of sand into the pipes, the yield was considerably below this average, not often exceeding and frequently falling below 2 cubic yards of sand per min. during the time actually occupied in pumping.

Including the time occupied in turning the boat and emptying the bins, the least average result of an entire month's work was obtained in November, 1871, during which period, .734 of a cubic yard per min. was removed. During the month of February, 1872, 1.127 cubic yards per min. were removed.

The greatest quantity removed on any one day was 770 cubic yards from 6.57 A. M., to 5.07 P. M., or at the rate of 1.26 cubic yards per min.

The manner of conducting the dredging may be briefly described as follows:

The steamer, with the suction pipes up, first crosses the bar to the outside, then turns around and steams slowly over the bar with just sufficient speed to maintain steerage way, lowering the pipes and starting the pump as soon as the outer edge of the bar is reached. Arriving at the inside, the pump is stopped, the pipes raised, and the steamer turned around again. She then crosses slowly to the outside, pumping as before, and the quantity of sand discharged into the bins during these two passages over the bar, is a load, whether great or small.

While the steamer is turning around on the outside, preparatory to taking in another load, the side gates of the bins are opened, the suction pipes are raised from the bottom, and the pump is run at full speed on clear water.

By this means, assisted to some extent when necessary by men in the bins with hoes, the sand is all discharged into deep water by the time the steamer has again reached the outer edge of the bar, when the dredging is resumed.

The time required to turn the steamer twice is 12 to 13 min., of which 6 to 6½ min., or the time occupied in making the turn on the inside, is lost, as neither the work of dredging the sand nor discharging it from the bins, is in progress during that interval.

GENERAL OBSERVATIONS.

1. The average cost of dredging and dumping the sand for the whole period of 7 months was 53¼ cents per cubic yard; the least cost during any entire week was 19 cents per cubic yard; and the least cost for any one day was on May 14, 1872, when 770 cubic yards were removed in 10⅙ hours at a cost of only 13 cents per cubic yard, the time actually occupied in pumping being only 5¾ hours.

2. The amount, exclusive of cost of pump and outfit, actually expended by the United States during the 7 months, in removing 36,969 cubic yards of sand, was $19,870, and the number of hours occupied at the bar in doing the work was 622¾. The number of hours lost, including Sundays paid for, and estimating the days at 10 hours each, was 1,347¼.

This lost time, with the exception of 16 days chargeable to the boat, and deducted from the charter, was nearly all occasioned by bad weather and low water on the bar. There were a few stoppages of short duration to repair the pumping apparatus. Assuming that no time had been lost from bad weather or insufficient depth of water, or, in other words, that operations had been carried on in still water with a boat that could work during all stages of the tide, the dredging and dumping would have cost less than 17 cents per cubic yard. This is an average estimate covering the entire period of 7 months, during a large portion of which, before the suction had been properly adjusted, the proportion of sand raised was considerably below the average, and greatly below the maximum results.

3. It may be remarked, in this connection, that there would be some difficulty in managing a boat of very light draft, in such a heavy sea and strong current, as usually prevail upon most of the ocean bars on our Atlantic coast, and in dredging upon very shallow ocean bars, even a boat built expressly for and well adapted to the work, might of necessity draw too much water to work continuously through low tide, during the earlier stages of the operation.

4. During the first five months (November 1 to April 1) the average duration of a round trip, in taking in and discharging a load, was $29\frac{7}{10}$ minutes, of which only $17\frac{2}{10}$ minutes was occupied in raising sand. During the last month, when the channel was longer, the round trip occupied 30½ minutes, 18 minutes being occupied in raising sand.

The average duration of a round trip during the 7 months was 38⅛ minutes, and the average time per trip occupied in dredging was $17\frac{6}{10}$. The pump was, therefore, actually employed in raising sand only 58 per cent. of the whole time spent at work upon the bar.

5. In dredging in still water this large percentage of lost time would be saved, as the dredging could go on continuously into scows or lighters on either side the dredging boat. There are localities where the dumping scows could doubtless be dispensed with, and the sand conveyed in spouts, or forced through pipes directly to the dumping ground. Of this character, as a class, are narrow streams having their channels near the shore, and many of the harbors on our Northern lakes, where the bars occur in a narrow water-way between parallel piers. In the former case the sand could be discharged on or near the shore, and in the latter over the piers on either side.

6. With a centrifugal drainage pump, sand can be easily discharged at a height of 30 ft. above the level of the water; and when the distance to which it has to be conveyed is so great that open troughs from the discharge pipe to the dumping ground, cannot have sufficient inclination to secure a free flow of the sand and water, it would be necessary to make the discharge through pipes, increasing the power expended in proportion to their length, so as to insure a velocity that will transport the sand and prevent choking. The pump itself should in all cases be placed as low as possible, and it would generally be practicable to locate it from 3 to 5 ft. above the surface of the water.

The cost of dredging under those circumstances, with a 9 in. pump, would probably not exceed 10 to 11 cents per cubic yard, inclusive of running expenses, wear and tear of machinery, and all stoppages for repairs and other contingencies. Indeed, assuming the pump on St. John's Bar to have worked continuously in raising sand, 10 hours per day, except Sundays, during the month of May, 1872, with the same average results per hour actually attained while pumping, thus charging the six working days of each week with the expense of seven, the cost of raising the sand into the bins wonld have been only $8\frac{6}{10}$ cents per cubic yard, and if it could, at the same time, have also been continuously discharg-

ed to the dumping ground, through either open troughs or pipes, no additional expense, except a trifle for increased power, would have been incurred. There were, moreover, constant losses encountered on the bar while actually pumping, which would not occur in still water, and of which no account has been taken, due to the pitching of the boat, which frequently lifted the ends of the suction pipes from the bottom. It is, therefore, considered safe to estimate the cost of removing sand at 10 to 11 cents per cubic yard, when the conditions are such that the work of raising the sand and discharging it to the dumping ground can be carried on simultaneously and continuously.

ON THE THEORETICAL MINIMUM OF FUEL REQUIRED TO PRODUCE ONE TON OF PIG-IRON.

From the "Journal of the Iron and Steel Institute."

Whether the actual amount of heat required in the production of a given quantity of pig-iron has been stated with rigid precision in the present work or not, there is no doubt whatever that there is absorbed in the process a certain number of calories or heat units, and no more.

The difficulty of defining what this quantity really is, arises, not from there being any variation or irregularity in the smelting operation itself under a given condition, but from the next to impossibility of correctly ascertaining the value of the factors which constitute the elements of the calculation.

Notwithstanding this uncertainty, I am of opinion that a sufficiently accurate estimate of the measure of heat required has been made to serve all practical purposes. The absorption of heat in the blast furnace, however, is not one of a simple character, *i. e.* it means something more than a conversion of a known number of calories into their equivalent of fuel, by even the most accurate knowledge of the calorific power of such fuel, and its power to do a certain quantity of work implied by the mere absorption of heat. As we have seen, the full calorific of the carbon which is burnt in the blast furnace has a restriction placed upon it by the chemical conditions attending the reduction of an oxide of iron.

Under these circumstances, it becomes necessary not only to ascertain how much heat is required for smelting iron, but we must know the precise extent to which we can look to our fuel for supplying that heat, and lying within this second branch of the inquiry, because, dependent upon it, is the subordinate one of how much of such heat must be obtained at the expense of burning fuel in the furnace, and how much may be derived from the less expensive source of having it communicated to, and conveyed by, the blast.

It has happened, upon more occasions than one, in recent discussions before the Iron and Steel Institute, that the question has been raised as to the minimum of fuel necessary for the production of a ton of iron, and what is the maximum useful temperature the air driven into the furnace can have.

No doubt much that can be said on these important questions has been already explained in these pages, but as the subject is one to which members of the Institute still address themselves, in their discussions, I have deemed it advisable to dedicate a distinct section to its consideration.

Before any attempt is made to ascertain how the necessary heat has to be commanded, we must agree on the exact quantity which is required, and to do this, an assumed condition of the fuel, of the atmosphere, and of the ironstone and limestone, must be taken as the basis of calculation.

The following will be adopted as forming the groundwork of the estimate:—

Durham coke, 100 parts, considered as consisting of —
Ash and sulphur 5 per cent, water 2.5 per cent, carbon 92.5 per cent.
One ton of iron will require—
Limestone 11 cwt., composed of 6.16 Ca O 4.84 CO_2.
Ironstone of Cleveland, calcined—49 cwt. composed of 18.6 Fe, 9.00 O, and 21.4 cwt. of earths, P. S., etc.

		cwt.
Slag will weigh—ash from the quantity of coke used	....	1.10
Ca O from limestone	....	6.16
Earths from ironstone	21.4	
Less bases, etc., taken up by pig iron, and evaporated in fume	.74	
		20.66
		27.92

Applying these figures to the coefficients of absorption already accepted, we have:—

		Cwt.		Units.
For Class I., No. 1.	Evaporation of H_2O in coke	.58 ×	540 =	312
" No. 2.	Reduction of Fe_2O_3	18.60 ×	1,780 =	33,108
" No. 3.	Carbon impregnation	.60 ×	2,400 =	1,440
" No. 4.	Expulsion of CO_2 from $Ca\,CO_3$	11. ×	370 =	4,070
" No. 5.	Decomposition of CO_2 in $Ca\,CO_3$	C 1.32 ×	3,200 =	4,224
" No. 6.	Decomposition of H_2O in blast	H .05 ×	34,000 =	1,700
" No. 7.	P_2O_5, SO_3, and SiO_2 reduce, say	.....	=	3,500
" No. 8.	Fusion of pig iron	20. ×	330 =	6,600
" No. 9.	Fusion of slag	27.92 ×	550 =	15,356
				70,310
Class II., No. 10.	Transmission through walls		3,600	
" No. 11.	Carried off by tuyere water		1,800	
" No. 12.	Expansion of blast, etc.		3,700	
				9,100
				79,410

This number, 79,410, is exclusive of the quantity of sensible heat carried off in the escaping gases, which varies, of course, with their temperature and weight; hence the absorption due from this cause will be provided for in the subsequent stages of the calculation.

In the data given above, the air is taken as somewhat drier than formerly (Section XXVIII.), and the P, S, and Si in the iron as the smallest quantity usually found in Cleveland pig metal.

It may be observed that the heat absorbed in the various steps of the process was deduced, in most cases, not alone from the direct experiment of different authorities, but the amount was checked by the heat evolved by the carbon in its ascertained state of oxidation, according to the data determined by the most accurate physicists. It will be seen the difference between the two sides of the accounts (*vide* Section XXVIII.) varied from 1,560 to 3,743 units, which, in 93,455, shows a discrepancy of only from 1.6 to 4 per cent. In the same section, a table, designated one of "constant requirements" was given, in which, when smelting Cleveland stone,

28.92 cwts. coke were used per ton of iron, and blast at	.....	485° C.
22.32 " " " "	.....	485° C.
22.00 " " " "	.....	780° C.

Notwithstanding the different characters of these factors, the extreme variation in the heat absorption deduced from their values was within 1.9 per cent.

In the examinations of furnace workings, which have preceded, it was estimated that the weight of carbon per ton of iron as CO_2 in the gases could not exceed 6.58 cwt., this quantity representing the deoxidation and carbon-impregnation of 18.6 cwt. of Fe, which was taken as that existing in one ton of pig-iron obtained from Cleveland stone.

Admitting, then, 6.58 cwt. to be the maximum proportion of carbon per ton of iron, to be found in the gases as CO_2, we must seek to ascertain to what extent a given volume of CO can abstract the necessary O from an oxide of iron required for its conversion into CO_2. The further we can carry this absorption, and retain the heat it generates, the less carbon will be needed in the form of CO, and the more extensively can we substitute heat obtained from the combustion of coke, by that injected into the furnace along with the blast.

On referring to the greatest quantity of CO_2, given in this work, as observed in furnace gases, we find the following figures:—

FURNACE.	Cubic feet.	Vols. CO_2 per 100 Vols. CO.		
		Maximum.	Minimum	Average.
Exp. 123. Clarence	11,500	50	32	44
" 132. Clarence	25,500	45	34	40
" 142. Ferryhill	16,000	46	36	40
" 147. Ferryhill	33,000	43	30	36

In experiments 95 and 99, it was found that 100 vols. of CO, when mixed with 50 of CO_2, still possessed the power of reducing Cleveland stone, but it was so feeble that, in the former, in 5½ hours, only .9 of the original oxygen (42.8), or 2.09 per cent., was removed; and in the latter, in 11¼ hours, 4.3 (out of 42.8), or 10 per cent.; the recorded temperature being 417 deg. C. (782 deg. F.) Such a temperature as 417 deg. C. is higher than that which ought properly to be found in the escaping gases of a blast furnace, inasmuch as it would of itself involve a loss of heat which may be avoided. In the four trials just quoted it will be seen, however, that 45 to 50 vols. of CO_2 per 100 of CO have been observed in furnace gases, indicating, therefore, the power of CO to pass into the state of CO_2 to the extent of one-third its own volume, by absorbing O from Fe_2O_3. I possess no record of the actual temperature of the gases at the moment the samples containing this maximum of CO_2 were taken; indeed, this would be difficult, if not impossible, to ascertain, owing to the rapid fluctuations in the composition of the gases, and the slowness in the indications of any instrument which can be used for ascertaining the temperature. Judging, however, from a vast number of other experiments, it is highly improbable the average was below 330 deg. C. (626 deg. F.)

If a continuous stream of materials, mechanically and chemically speaking, the same, were flowing into a blast furnace, it is quite possible that the fluctuations, in respect to temperature and composition of the gases, would be removed. Practically, in furnaces, as they are at present constructed for taking off the gases, such a plan of charging could scarcely be pursued; nor am I prepared to admit that, were the uniformity spoken of attained, the average in these respects would much differ from what it now is, but my present object is to show what the consumption of fuel might be, were it possible to maintain the gases at the maximum of oxygen saturation, and minimum of sensible heat, indicated in the instances just quoted.

For this estimate, 100 vols. of CO will be taken as being accompanied with 50 vols. of CO_2, or by weight 100 of CO to 78.3 of CO_2. In using ironstone perfectly dry, and at the temperature of the atmosphere, it will be assumed that the heat of the escaping gases is as low as 275 deg. C. (527 deg. F.)

It must, however, be understood, that this assumption of a maximum saturation of oxygen, and minimum quantity of sensible heat in the gases, is merely made use of as a basis of calculation, for I shall hereafter show that, although it is easy to maintain even a much higher proportion of CO_2 than 50 vols. to 100 of CO in the gases, and it is also within our power to cool them to 275 deg. C., it is, in my opinion, quite impossible to command these two conditions simultaneously, and I shall also give experimental evidence to prove that as we gain by the one, we lose by the other, and *vice versa*. It must also be borne in mind that with such a large quantity of CO_2 in the gases (50 vols. to 100 CO), it required a temperature (Exps. 95 and 99) of 417 deg. C. (782 deg. F.) to remove the small proportion of oxygen just mentioned. At 275 deg., the temperature to be assumed in our calculation, it must be considered that the gases are not only practically, but are physically, saturated with oxygen, so far as they can be, when calcined Cleveland stone is the source of this element.

To proceed with the estimate, 6.58 cwt. of carbon which pass into the state of CO_2 for each ton of iron in the manner supposed is equal to 24.13 CO_2, and therefore affording, by oxidation, 52,640 units.

The actual weight of the gases containing 6.58 cwts. of C as CO_2, and consisting of CO and CO_2 in the proportion of 100 to 78.3 will be—

CO., 30.85 cwt.	containing.....	13.22 cwt.	C.
CO_2 24.13 "	do	6.58 "	"
54.98 containing C	.....	19.80 "	"
Of which the limestone supplied..		1.32 "	"
Leaving to be supplied by the coke		18.48 "	"
Carbon taken up by the iron.....		60 "	"
Total carbon in the gases derived from coke.....		19.08 "	"

and 19.04 of carbon is equal to 20.62 cwt. of coke containing 92.5 per cent. of this element.

The figures just given will enable us to proceed to compare the heat evolution with its appropriation upon the data already set forth, as well as to determine the temperature of the blast.

The total oxygen contained in the abovenamed quantities of CO and CO_2 is 17.55 in the one, and 17.63 in the other, or a total of 35.18 cwt.; hence it would appear that

the oxygen is equally divided between its combination of CO and CO_2.

From the total oxygen in gases, viz.,		35.18 cwt.
Deduct that derived from the Fe_2O_3, P_2O_5, SiO_2, and SO_3, in the ore, say	9.00	
Deduct that derived from the limestone	3.53	
" " moisture in blast	.40	
		12.92 "
Leaving the blast to supply		22.26 "
Add weight of nitrogen accompanying this O		74.52 "
Moisture		.45 "
Weight of blast		97.23 "

Weight of gases—Blast	97.23	
O from ore, limestone and H_2O	12.92	
C in gases	19.80	
H_2O from coke	.50	
		130.45 "

The total number of calories actually required will be those given in a previous page 79,410

The total number in 130.24 cwt., gases at a temperature of 275°C.

130.24 cwt. × 275°C. × .24SH 8,610

88,020

The heat evolved will be as follows:

C to CO	13.22 cwt.				
Less than in limestone	2.32 "				
	11.90	×	2,400	=	28,560
C to CO_2	6.58	×	8,000	=	52,640
	18.48				81,200
Left to be supplied by the blast					6,820

and $\frac{6{,}820 \text{ units}}{97.23 \text{ cwt.} \times .237 \text{ SH}} = 297°\text{C.}$ (527°F.)

The calculation works out to a lower temperature in the blast than any ironmaster knows will suffice in practice, when using so small a quantity of coke as 20.58 cwt. This is partly owing to the fact that the gases are taken below the usual temperature at which they leave the furnace, and partly due to the circumstance that they do not generally contain quite the full equivalent of CO_2.

These two corrections would bring up the temperature required in the air, but they do not, in any way, affect the weight of coke consumed, which, I much doubt, under the conditions assumed, will ever be reduced practically below 20½ cwt. per ton, and, to cover those irregularities so frequently mentioned upon other occasions, it is rather to be expected that for a ton of No. 3 iron it will more frequently be 21, or perhaps nearer 21½ cwt.

By the erection of enormous furnaces, and the adaptation of heating-apparatus of immense power, hopes have been expressed of reducing the consumption of coke to 18, or indeed to 17 cwt. per ton, of iron obtained from Cleveland stone. Now, the effect of this would be, that the CO_2, generated in the zone of reduction, would bear the ratio of about 96 parts, by weight, for each 100 of CO.* I do not pretend that a mixture consisting even of equal weights of CO and CO_2 (100 vols. CO to 64 CO_2) possesses no reducing power, but it must be very faint, and will require a higher temperature than that usually found in the gases leaving a blast furnace. This is inferred from the experiments referred to at the beginning of this section, seeing that pure CO only commences to exhibit marked action on Cleveland stone at 210 deg. C. (410 deg. F.)—*Vide* Exp. 17.

It does appear, however, that there is a certain amount of inconsistency in the simultaneous adoption of very large furnaces, and very high temperature of blast. The increase of dimensions afforded a reasonable prospect of permitting the gases to part with more of their sensible heat, and of becoming more thoroughly saturated with oxygen before leaving the throat, than happens when smaller dimensions are adhered

* *Vide* Table, Section XXXII., on Composition of Gases, using different quantities of coke.

to; and these actions are necessarily attended with a saving of fuel. When this augmentation in size is practicable, and very large capacity is efficacious in economizing coke, it is unnecessary to raise the temperature of the blast, inasmuch as the quantity of heat evolved from such a state of oxidation of carbon as I have supposed in the estimate, viz., 6.58 parts to CO_2 and 13.18 to CO, is equal to giving a ton of iron, as we have just seen, with a blast of very moderate temperature, viz., 297 deg. C. (527 deg. F.)

It may, no doubt, be argued that further action by a gas on an oxide of iron still possessing reducing properties, ceases, because it is cooled below that point where deoxidation is effected, and that, in consequence, it is reasonable to expect that raising the temperature of the blast would, by a general elevation of the heat of the gaseous and solid contents of the furnace, enable reduction to be effected by a mixture of CO and CO_2, which, at lower temperatures, is inert.

Of course, such a procedure would mean that the escaping gases were leaving the furnace in a more highly heated condition than before, and, hence, one advantage accruing from an enlargement of size would be at once defeated. This is a state of things, however, that would be unproductive of any real gain, *i. e.*, it would be useless to add a given number of heat units at the bottom of the furnace if the same number escaped at the top. The additional heat, therefore, to have any value, must have additional work to perform by adding a greater load of ironstone to the coke.

Inasmuch, however, as very much larger proportions of CO_2 than 50 of this gas to 100 vols. of CO have been shown to possess, under certain circumstances, well marked powers of deoxidizing Cleveland stone, it may be well to consider the nature of those circumstances with a view to see how far they can be applied to the blast furnace, when smelting this ore with coke.

In Exp. 81, CO was entirely converted into CO_2 by being passed slowly over a large excess of calcined Cleveland stone, at a temperature a little above that of melting zinc. The Fe_2O_3, however, only lost 5.25 per cent. of its O.

In Exp. 82, 100 vols. CO and 600 vols. CO_2 at a low red heat removed 5.8 per cent. of the O from Cleveland calcined ore.

In Exp. 84, 100 vols. CO and 220 vols. CO_2 at a heat just visibly red absorbed 11.7 per cent. of the O in Cleveland stone.

In Exp. 85, equal volumes of CO and CO_2 formed stable compounds with various ores, at a red heat. They were reduced to Fe O, and spongy metallic iron exposed to this mixture of gases at the same time, absorbed oxygen and became Fe O.

Now, there is no doubt the furnace itself may be made to imitate the action just mentioned, *i.e.*, the issuing gases may contain a very much larger proportion of CO_2, than that expressed by the ratio of 50 vols. to 100 of CO.

Exp. 764. Immediately after charging into the Wear furnace (17,500 cubic ft.) 25 cwt. of coke, 12 cwt. of limestone, and 54 cwt. of calcined Cleveland stone, in the order in which they are mentioned, the gases were found to consist of

CO_2 8.8	Co_2 30	66 vols. for 100 vols. CO, or
CO 28.7	" 48.06	parts by weight to 100 CO.
H 1.1		
N 61.4		
100 vols.		

All charging was now discontinued, and the gases were sampled at intervals of 20 min., and afforded the following results:—

		CO_2	CO	H	N		Vols.	100 Co by Vols.	= Co_2 by Weight.
Exp. 765.	end 20".	10 7	29 2	3.3	56 8	=	100	36.64	57.38
" 766.	" 40".	12.3	25.2	3.8	58.7	=	100	48.81	76.44
" 767.	" 60".	22.2	16.9	2.1	58.8	=	100	131.36	205.89

To verify the correctness of these numbers, Exps. 766 and 767 were repeated.

Now, really what takes place under such conditions as those just described, I take it, is this. After charging, any soft coke is immediately attacked, and C is dissolved at the expense of CO_2. At the Wear Works, this happens in a more striking way, perhaps, than usual, owing to the coal used there giving a softer coke. This action, however, ceases in time, and the CO_2 produced by deoxidation, not passing through any fuel, owing to the order in which the materials were charged, begins to increase as is seen in Exps. 764 and 765, in which the ratio has risen from 30.66 to 36.64 vols. This continues, and, by the end of the hour, it has become 131.36 vols., or above two

and a-half times that which was adopted as the basis of the calculation to obtain the theoretical *minimum* of coke.

A great deal, however, of the heat evolved by the extraordinary proportion of CO_2 is lost, for being generated so near the point of exit, it is carried away in the escaping gases, as may be seen by the following:—

	Clarence.		Temp.	Ceased charging during	Temp.	Rise.
		Cubic ft.	°F	h. m.	°F	°F
Exp 768.....	No. 4 furnace ..	25,500	579	1 26	816	237
" 769.....	"	"	564	1 20	814	250
" 770.....	"	"	593	1 23	797	204
" 771.....	"	"	536	1 40	788	202
" 772.....	No. 5 furnace ..	25,500	608	1 30	844	236
" 773.....	"	"	644	1 20	834	190
" 774.....	"	"	586	2 30	887	301
" 775.....	"	"	629	1 42	867	238
	Average rise...	....	....	1 36	....	232 = 129°C.

Now the ordinary composition of the gases of the Wear furnace, when working upon the burden mentioned above, was, by weight,

(*V.* Exp. 536) CO_2 14.9, CO 28.2, N 56.9 = 52.8 parts by weight of CO_2, to 100 CO.

100

And, in round numbers, the calories evolved by burning the fuel used per ton of iron, viz., 23.5 cwt. (*v.* Exp. 536) would, when the gases were emitted, containing CO_2 to CO as 205 to 100, be above 30,000 more than when the oxides of carbon were as 52.8 to 100.

Practically, however, any heat thus evolved and afterwards absorbed is in part entirely wasted, because the moment charging is resumed the accumulation enables the highly-heated CO_2 to act more vigorously on the freshly added coke, and that coke acts quickly in this way may be inferred from the following line of argument.

In Exp. 751, data were given, which showed, bulk for bulk, ironstone possessed twice the power of coke in intercepting heat when a current of hot air was blown through it, there being under the circumstances of the trial no chemical action. Now, as ironstone is about double the weight of coke, it follows that, weight for weight, their heat-intercepting powers are about equal. It may, therefore, be expected that apart from chemical action, if 24 cwt. of coke were introduced into a furnace it would affect the temperature of the gases by simple refrigeration to the extent of something like one-half the extent of that of 50 cwt. of ironstone, including its flux.

Exp. 776. Twenty-five observations were made on the temperature of the gases five minutes after charging 24 cwt. of coke. The fall varied considerably, but the average was 38 deg. F. (21 deg. C.)

Exp. 777. A similar number of trials was made after charging 50 cwt. of mine, etc. Here, too, the variation was great, but the average cooling amounted to only 23 deg. F. (13 deg. C.), also observed five minutes after charging.

Now, under no circumstances can the introduction of coke into the reducing zone be accompanied by an evolution of heat. Undoubtedly, it is otherwise to some extent, but not a large one with ironstone; but however this may be, five minutes would not suffice to bring it up to the necessary temperature to induce strong chemical action; so that, in the twenty-five experiments just described, we may regard it as occupying the same neutral position it did when hot air was blown through it. Admitting this, as regards the ironstone, there seems no alternative but to ascribe the increased rate of cooling exercised by the coke in the blast furnace to chemical action, *i.e.*, the high temperature of the gases expends itself by causing CO_2 to act on the carbon.

I proceeded to examine the correctness of this supposition by the following series of

experiments. The fall of temperature was ascertained to be as follows, after the dinner time, during which, charging had been discontinued :—

	Temp. after dinner.	After charging three rounds, weighing 403 cwts. of coke, mine, and flux.	Time occupied.	Fall.
			h. m.	
Exp 778	816°F.	572°F.	1 10	244°F.
" 779.....	816	552	1 30	264
" 780.....	797	636	0 50	161
" 781.....	788	552	1 0	236
" 782	834	593	1 0	241
" 783.....	834	629	1 40	205
" 784.....	887	617	0 50	270
" 785.....	867	624	1 0	243
			Average fall...	233 = 129°C.

So that the gases have fallen in temperature, after 403 cwt. were introduced to fill the furnace again, almost exactly what they had risen while charging was discontinued.

The next step was to ascertain the nature of the change which had taken place in the composition of the gases, and this is set forth in the following analyses :—

		CO_2.	CO.	H.	N.	Vols.
Exp. 786.	After ceasing to charge.....	8.8	28.7	1.1	61.4	= 100
" 787.	Before starting to charge after an interval of 1¼ hr.....	22.2	16.9	2.1	58.8	= 100
" 788.	After first round.....	10.9	28 6	3.2	57.3	= 100
" 789.	" third "	8 3	30 4	2.2	59 1	= 100
" 790.	" fourth "	11.3	23.8	4 0	60.9	= 100

In order to prove how immediately the high temperature, acquired during the dinner hour, causes the CO_2 to act on the coke, the following experiments were performed at the Wear furnace (17,500 cubic ft.):

	Hour.	Charged.	CO_2	CO.	H.	N.		Vols.	Vols. CO_2 per 100 CO.
Exp. 791.	11.40	.....	11.6	28.1	1.8	58.5	=	100	41
" 792.	12 5	Charged 1 round coke, ironstone, &c., furnace full....	10 7	not ascertained.					
" 793.	12.30	.....	11 7	do.					
" 794.	12 55	.....	19 1	18.3	0.2	62 4	=	100	105
	1.10	Charged 52 cwt. coke.							
" 795.	1 12	No further charging.....	13 1	22.7	2.7	61 5	=	100	58
" 796.	1.28	do.	11 1	25 2	3.1	62.4	=	100	44
" 797.	2.10	do.	12.7	23.5	2 0	61.8	=	100	54
" 798.	2.55	do.	22.7	17.8	..	59 5	=	100	128
" 799.	3.40	do.	15 8	16.4	1 5	66 3	=	100	96
" 800.	4.5	do.	11.8	26.7	1.2	60.3	=	100	44

In the above experiments, the furnace, on being filled up with the ordinary round, viz., coke 26 cwt., mine 54 cwt., limestone 12 cwt., the gases exhibit their ordinary composition. Nothing was filled during 1 hour and 5 min., by which time the CO_2 had risen from 41 vols. to 105 vols. per 100 CO. Two rounds, weighing 52 cwt. coke (Exp. 795), were then let at once into the furnace, and immediately after the CO_2 fell to 58 vols., and in 18 min., to 44 vols. per 100 of CO. The CO_2 now began to rise again, and reached, in Exp. 798, 128 vols. per 100 of CO. No more ironstone being introduced, the CO_2 now began to fall off in quantity, and in 3 hours 55 min., had fallen to 44 vols. per 100 of CO.

To ascertain what the temperature really was, which sufficed for this speedy action of coke on CO_2, the following trials were instituted, also at Wear furnace.

After Exp. 802, when the coke was introduced the temperature of the gases would probably be 850 deg. F., and the CO_2 to CO most likely as 50 to 100 vols. In three minutes after the coke entered the furnace, the temperature of the gases fell to 625 deg. F., and they immediately afterwards contained CO_2 and CO in the ratio of probably 30 vols. to 100.

	Hour.		CO_2	CO	H	N	Vols.	CO_2 per 100 vols. CO.	Temp.
Exp. 801.	12	.....	12.5	26.4	3.8	57.3	= 100	47 3	
" 802.	12.13	.....	12 0	not ascertained.					800F
	12.20	let in 26 cwt. coke...							
" 803.	12.23	.....	....	....	....	....	....	....	625F
" 804.	12.30	.....	10.7	30 6	1.3	57 4	= 100	34.9	
" 805.	12.45	.....	8.4	not ascertained.		probably		30.	
" 806.	1.35	.....	16.7	21.0	1.5	60.8	= 100	79.5	1025F

In this way, it will be perceived that there is a constant struggle between the formation of CO_2, accompanied by high temperature and the reduction of the CO_2 thus generated, back again to CO, and which latter change is, of course, effected at the expense of heat. Between these two extremes there is a middle course in which time is an important element, for the quantity of CO_2 formed, and its attendant heat, will depend upon the relative rapidity with which CO can absorb O from the Fe_2O_3 under treatment, and that at which the resulting CO_2 takes up a fresh equivalent of C. In the case of Cleveland stone the mean of the two conflicts, in my opinion, is generally reached even before the CO_2 exists in the proportions which constitute the basis of the calculation just given, viz., 50 vols. of this gas to 100 vols. of CO, and when the temperature of the escaping gases consequent upon this proportion of CO_2 is about 330 deg. C. (626 deg. F.) I will hereafter give a remarkable instance of a very different rate of fuel consumption from that obtainable when the ironstone of Cleveland is the ore used in the blast furnace, and an attempt will be made to explain the cause of this modification in the progress of the action.

If, then, accompanying a given quantity of heat required for the production of a ton of pig iron, a certain quantity of CO must be present, it follows as a matter of course that any heat contributed by that contained in the blast will be useless, which when it is added to that generated by the formation of the CO rendered necessary by the nature of the process, the sum of the two exceeds that shown to be required. I have endeavored to prove what becomes of this surplus of heat, viz., that it passes off in the escaping gases, or it is expended in the CO_2, formed in the upper zone of the furnace, absorbing C.

The second branch of the inquiry, viz., the *maximum* temperature the blast ought to have to permit its profitable application, is dependent entirely on the extent to which the permanent state of oxidation of CO to CO_2 is maintained. If it can be carried to such a point that the whole of the CO_2 due to deoxidation and carbon impregnation escapes as such, and that 100 vols. of CO can restrain the oxidizing influence of 50 vols. CO_2, then I have shown with the escaping gases at a temperature of 275 deg. C. the air only requires to be heated to 297 deg. C. (527 deg. F.) to supply the deficiency of heat.

To effect this, however, the whole of the conditions must be favorable in the highest degree. The coke must be of that quality as to resist the power of heated CO_2 to dissolve it, and the gases must be prevented acquiring a temperature beyond 275 deg. C. All this is attended with much difficulty, so much, indeed, that it far oftener happens that the total C escaping is not much above 6.0 cwt. as CO_2, and the CO_2 to CO is as 40 or 45 vols. to 100, or as 62.7 to 100 by weight, with a temperature of 330 deg. C.

Now the difference between such a state of working, and that which has been hypothetically assumed as the basis of the calculation which brought out 20.62 cwt., burnt with the blast at 297 deg. C., as being the quantity of coke required to produce one ton of iron, will be somewhat as follows. There will be a loss of .58 cwt. of carbon burnt to CO instead of CO_2 equal to (.58 × 5,600) 3,248 calories, and the gases escaping at 330 deg. C., instead of 275 deg., will represent about 1,700 units, making together, say 4,900 to 5,000 units. If the coke remained as it was, viz., 20.62 cwt., there would be nearly 5,000 units to be conveyed by 97.23 cwt. of blast, which represents an increase of temperature of about 190 deg. C., making the total 502 deg. C. (935 deg. F.) In practice, however, the diminution of CO_2 in the gases affects the quantity of carbon to be burnt at the tuyeres, where, at all events, the heat evolved is more readily rendered available by interception; hence the actual loss arising from the causes now under consideration, is more generally to be met partly by a little more coke as well as by an increase in the temperature of the blast.

The manner in which defects of so varying a character have to be provided for, scarcely admits of very precise estimation. It must, in consequence, rather be the result of general experience than that of any formula of figures. Regarded in this way, I would give it as my own opinion that taking the ordinary run of Durham coke and Cleveland ironstone, the ironmaster who produces a ton of No. 3 iron with 21¼ cwt., with the blast heated to 500 deg. C. (932 deg. F.) may consider himself as working very closely up to those limits of economy which are prescribed by the nature of the materials he is operating upon.

Independently of any increase of temperature in the upper portion of the furnace, the use of the hot blast introduces a change in the relation between the solid and gaseous contents, which will have the effect of accelerating the tendency towards an equalization of temperature of the two. By this action the ore is more speedily heated, and the gases, in consequence, are more quickly saturated with oxygen gas. This arises from the longer retention in the furnace of the carbonic oxide evolved by the combustion of a given weight of coke, and this happens in the following way:—

Let us imagine that a furnace of 6,000 cubic ft. has conveyed into it, by means of the hot blast, 14,400 cwt. heat units per ton of iron, over and above the number it would receive were the air driven into it at the temperature of the atmosphere. This is equal to the heat from 6 cwt. of carbon burnt to CO, which C can therefore at once be withdrawn from the fuel introduced with the charges.

This quantity of carbon represents in resulting gases (C, 6; O, 8; N, 26.8) 38.8 cwt.

This is equivalent to a reduction of nearly 10 per cent. in the volume of the gases flowing up through the contents of a furnace using originally 60 cwt. of coke per ton of iron. During the time, therefore, each cwt. of carbon is engaged in fusing the iron and slag in the hearth, while in the act of being converted into carbonic oxide, the CO thus formed is retained, by the diminution in its volume, one-tenth longer period in the furnace, and by so much its contact with the materials it has to heat and to reduce is prolonged. This effects a further saving of fuel, and, in consequence, a further reduction in the volume of gas is accompanied again by an addition to the time of retention of this gas in the furnace. This is continued until the fuel is diminished in quantity, as far as a furnace of the supposed dimensions is capable of producing a ton of iron, which is not reached until the gases in bulk are a mere trifle above one-half what they were when the blast consisted of cold air, instead of with the supposed addition of the 14,400 units in question.

It is clear, under such circumstances, the reducing gases will be twice as long in contact with the ore they have to reduce, and with the materials they have to heat, as when cold blast is employed.

That it is time alone which effects the change, and no mysterious virtue in the heat of the blast, is proved, I submit, beyond all doubt, by the fact that precisely the same results, in point of fuel consumed, are obtained by a suitable enlargement in the dimensions of the furnace, by which prolonged contact between the gases and solids is, in like manner, secured.

Before concluding this section on the economy of fuel, I would briefly notice a plan recommended by Mr. C. Schinz, in his work on the blast furnace,* and which he has patented under the title of "A process for partly eliminating the nitrogen in the products of combustion."

Half the schemes intended to improve the smelting of iron are propounded by persons who neither by practice nor study have endeavored to make themselves acquainted with the principles upon which it is dependent. To this class, Mr. C. Schinz is a striking exception, for he has taken infinite pains to convince himself of the soundness of his project, and yet I entertain little hope of its sharing a different fate from many of its less studied predecessors.

The plan, in reality, consists of forcing carbonic oxide, obtained in one or two different ways, into the blast furnace; but, apparently, the one, preferred by the inventor, consists in heating refuse coke and carbonate of lime in retorts similar to those employed in a gas manufactory.

I am at a loss to know from what quarter refuse coke has to be obtained, in sufficient quantity, to constitute an important saving in the fuel consumed in iron smelting; but, omitting this difficulty, I cannot see how an elimination of nitrogen, partial or entire, can produce any notable economy in the quantity of fuel required for the process.

* "Dokumente betreffend den Hohofen."

The production of CO, containing the necessary quantity of carbon, would require an enormous establishment, and a great expenditure in labor and in heat, exceeding the value of its equivalent weight in coke, and, after all, we obtain a fuel, CO, which the nature of the process would not permit us to oxidize at the tuyeres. Independently of all this, calculation would show that the absorption of heat in decomposing carbonate of lime and resolving its CO_2 into CO, is so great as to present an insuperable barrier to our being able, profitably, to avail ourselves of Mr. C. Schinz's proposal.

THE INFLUENCE OF LINES OF METAL IN BUILDINGS IN DETERMINING THE DIRECTION OF DISCHARGES OF LIGHTNING.

By EDWARD E. QUIMBY.

Written for Van Nostrand's Magazine.

Mr. Henry Wilde's views upon this subject, exhibited in his paper read before the Manchester (Eng.) Literary and Philosophical Society, are of course correct; but they are not entirely new in this country, similar opinions and conclusions having been publicly expressed by the late A. M. Quimby in the course of a series of popular lectures upon "electricity and the laws of thunder-storms," delivered as long ago as 1834.

The lightning-rod business, so-called, in the United States, is pursued by a large class of itinerants, who not only evince gross ignorance of electrical science, but frequently practise the most unprincipled rascality. A few practitioners, who have studied the subject philosophically, pursue the business as a profession, not selling lightning-rods as an article of merchandise, but administering them, or, in other words, prescribing their arrangement and personally supervising their application to buildings precisely as regular physicians prescribe for their patients.

Among the latter class, the practice of connecting the lightning-rod with lines of metal within the building, such as line shafting in mills, or gas, water or steam pipes, has been carefully observed for nearly forty years.

In connecting with a lead pipe, care is taken to secure a considerable area of surface contact, to prevent the pipe from being melted by lightning at the point of junction with the rod. This precaution must be especially important in England, where lead pipes are sometimes used for the gas service.

The solution of the practical problem, how to protect a given building from lightning, is governed by the consideration of that invariable law of electricity, which determines the selection by a discharge of lightning of the path of least resistance.

Damage ensues within a building where lightning passes from one good conducting substance to another through an intervening imperfect conductor. If the detached masses of conducting matter are linked together by the introduction of conducting substances between them, the missing links, as it were, in the chain are supplied, and a path is established upon which a discharge of lightning can pass without causing damage. If, instead of seeking out the various metallic substances in a building and connecting them together, a parallel line of conducting matter, of sufficient capacity, is established, the same result is accomplished. But the building occupies a very short section of the track of a discharge of lightning, while the gas and water pipes within it communicate with systems of underground mains of perhaps miles in extent. The perfect connection of the lightning-rod with such pipes, therefore, is an indispensable requisite to the establishment of a complete path for the discharge of lightning.

A writer in the current number of the "Franklin Institute Journal," makes some pertinent and timely remarks upon defective lightning rods, which are especially noteworthy as emanating from a city which is one of the centres of the "patent lightning-rod" trade.

The article in question noticeably omits any allusion to the practical researches and experience of the late Messrs. King of Boston, and Quimby of New York, and yet forcibly presents views upon the subject of protection from lightning which King and Quimby propounded forty years ago, and

which have since been acted upon in this country with complete success.

The writer, however, also expresses some extreme opinions, which, while erroneous, derive importance from their association with so much that is sound and valuable.

The proposition, that because where there are gas or water pipes, they must be connected with the lightning-rod, therefore, where there are no such pipes or their equivalent, the lightning-rod is useless, is untenable.

Uncontradicted, such a statement will produce in the minds of average readers a general distrust in the efficacy of what experience has demonstrated may be made a perfect safeguard under all circumstances.

To secure absolute protection from lightning, the rod must be so arranged as to present, either in itself or in conjunction with masses of metal about the building, continuous lines of conducting matter, which, *relatively to the building and its contents*, constitute the paths of least resistance in any and all directions, and in all the various circumstances and conditions under which the building may be exposed to the effects of lightning.

A discharge of lightning has a fixed destination; it occurs because of the juxtaposition of two bodies or masses of matter which are oppositely electrified, and its course lies between them. It invariably selects the shortest attainable path upon the best conducting matter present. It is, perhaps, made up of the electricity of a cloud thousands of acres in extent. The entire cubic contents of a building constitute but an insignificant fraction of the mass of matter affected by the electrical disturbance and contributing to the discharge. So far, therefore, as regards by far the larger portion of the discharge, the building is merely an incident in its path, and the function of the lightning-rod is simply to present a superior line of conducting matter in all directions. The ground connection of the rod must be with the best conducting material which may be present. If it is a gas or water main, with that; if a stream of water, with that; if merely moist earth, then with that. The terminations of the rods must penetrate the earth below the building just as they must project above the building, so that the rod may be ready to receive the lightning from whatever quarter it may come, and to carry it off to whatever direction it may tend.

A portion of the discharge is required to restore the equilibrium of the building itself, which, in common with all other matter in the vicinity, is in a state of electrical excitement induced by the proximate thunder-cloud. It is, therefore, essential that the lightning-rod should be established in the most intimate connection with all parts of the building, and especially with considerable masses or lines of metal about the building, so that the electricity required to restore its equilibrium may be taken from or communicated to it in so many different places at the same time, and in such small quantities as to be nowhere sufficient to produce any mechanical disruptive effects.

The conducting capacity of a rod depends upon the extent of its solid cross-section. Increasing its surface by flattening it out, increases, if anything, the resistance which it opposes to electricity in motion. Rods made of twisted, corrugated, or tubular sheet metal, are also inferior to a solid bar, because they are less substantial and less capable of durable application.

There is no occult power in the lightning-rod itself, whatever may be its form or its material, which makes it a sovereign specific against damage by lightning. Mr. Wilde recites an instance of the destruction of a church by a discharge of lightning, which left a copper rod for an iron gas pipe.

It is only when the lightning-rod is adjusted to the requirements of the building by the exercise of intelligent judgment and appreciation of the laws of electrical manifestations, that it can be relied upon as a perfect safeguard from lightning.

The entire anthracite coal tonnage of the Philadelphia and Reading Railroad Company, for the year 1871, was 4,026,216 tons, of which 3,716,584 tons were from the Schuylkill, Shamokin and Mahanoy regions. This company, under the name of the Philadelphia and Reading Coal and Iron Company, own about 70,000 acres of coal lands costing nearly $12,000,000.

The Chicago and Michigan Lake Shore Railroad Company are making large preparations for docks and warehouses at Pentwater.

THE IMPROVEMENT OF THE STEAM ENGINE AND THE EDUCATION OF ENGINEERS.*

By PROFESSOR THURSTON.

Having rapidly sketched the history of the steam engine and of some of its most important applications, we may now take up the question: What is the problem, stated precisely and in its most general form, that engineers have been here attempting to solve?

After stating the problem, we will examine the record with a view to determine what direction the path of improvement has taken hitherto, and, so far as we may judge the future by the past, by inference, to ascertain what appears to be the proper course for the present and for the immediate future.

Still further, we will inquire, what are the conditions, physical and intellectual, which best aid our progress in perfecting the steam engine.

This most important problem may be stated in its most general yet definite form, as follows:

To construct a machine which shall, in the most perfect manner possible, convert the kinetic energy of heat into mechanical power, the heat being derived from the combustion of fuel, and steam being the receiver and the conveyer of that heat.

The problem embodies two distinct and equally important inquiries.

1. What are the scientific principles involved in the problem as stated?

2. How shall a machine be constructed that shall most efficiently embody and accord with, not only those scientific principles, but also all of those principles of engineering practice that so vitally affect the economical value of every machine?

The one question is addressed to the man of science, the other to the engineer. They can be only satisfactorily answered, even so far as our knowledge at present permits, after studying with care the scientific principles involved in the theory of the steam engine under the best light that science can afford us, and by a careful study of the various steps of improvement that have taken place and of accompanying variations of structure, analyzing the effect of each change, and tracing the reasons therefor.

The theory of the steam engine is too important and too extensive a subject to be treated here in even the most concise possible manner.

I can only attempt a plain statement of the course which seems to be pointed out by science as the proper one to pursue in the endeavor to increase the economical efficiency of steam engines.

The teachings of science indicate that success in economically deriving mechanical power from the energy of heat motion, will, in all cases, be the greater as we work between more widely separated limits of temperature, and as we more perfectly provide against losses by dissipation of heat in directions in which it is unavailable for the production of power.

Scientific research has proved that, in all known varieties of heat engine, a large loss of effect is unavoidable, from the fact that we cannot reduce the lower limit of temperature, in working, below a point which is far above the absolute zero of temperature; far above that point at which bodies have no heat motion; the point corresponding to the mean temperature of the surface of the earth is the lower limit.

The higher we carry the temperature of the steam when it enters the steam cylinder, and the lower that at which it arrives before the exhaust occurs, the greater, science tells us, will be our success, provided we at the same time avoid waste of heat and power.

Now, looking back over the history of the steam engine, let us rapidly note the prominent improvements and the most striking changes of form, and thus endeavor to obtain some idea of the general direction in which we are to look for further advance.

Beginning with the machine of Porta, at which point we may first take up an unbroken thread, it will be remembered that we there found a single vessel performing the functions of all the parts of a modern pumping engine; it was, at once, boiler, steam cylinder, and condenser, as well as both a lifting and a forcing pump.

The Marquis of Worcester divided the

* A paper read before the Polytechnic Association of the American Institute, being the concluding portion of a lecture addressed to mechanics and engineers, at the Stevens Institute of Technology, February 1872.

engine into two parts, using a separate boiler.

Savery duplicated that part of the engine of Worcester which performed the several parts of pump, steam cylinder and condenser, and added the use of water to effect rapid condensation.

Newcomen and Cowley next separated the pump from the steam engine proper, and in their engine as in Savery's, we noticed the use of surface condensation first and subsequently that of the jet, thrown into the midst of the steam to be condensed.

Watt finally effected the crowning improvements, and completed the movement of "differentiation" by separating the condenser from the steam cylinder. Here this movement ceased, the several important processes of the steam engine now being conducted, each in a separate vessel. The boiler furnished the steam, the cylinder derived from it mechanical power, and it was finally condensed in a separate vessel, while the power which had been obtained from it in the steam cylinder was transmitted through still other parts, to the pumps, or wherever work was to be done.

Watt, also, took the initiative in another direction. He continually increased the efficiency of the machine by improving the proportions of its parts and the character of its workmanship, thus making it possible to render available many of those improvements in detail, upon which effectiveness is so greatly dependent, and which are only useful when made by a skilful workman.

Watt and his contemporaries also commenced that movement toward higher pressures of steam and greater expansion, which has been the most striking feature noticed in the progress of steam engineering since his time.

Newcomen used steam of barely more than atmospheric pressure and raised 105,000 lbs. of water one foot high with a pound of coal consumed. Smeaton raised the pressure somewhat and increased the duty considerably; Watt started with a duty double that of Newcomen and raised it to 320,000 foot-pounds per pound of coal with steam at 10 lbs. pressure.

To-day, Cornish engines of the same general plan as those of Watt, but worked with 40 to 60 lbs. of steam and expanding three or four times, do a duty probably averaging, with the better class of engines, 600,000 foot-pounds per pound of coal. The compound pumping engine runs the figure up to about 1,000,000.

The increase in steam pressure and in expansion since Watt's time, have been accompanied by a very great improvement in workmanship; a consequence, very largely, of the rapid increase in perfection, and in the wide range of adaptation of machine tools, by higher skill and intelligence in designing engines and boilers, by increased piston speed, greater care in obtaining dry steam, and in keeping it dry, until thrown out of the cylinder, either by steam jacketing or by superheating, or both combined; and it has further been accompanied by greater attention to the important matter of providing carefully against losses by radiation and conduction of heat.

The use, finally, of the compound or double-cylinder engine for the purpose of saving some of the heat usually lost in consequence of condensation from great expansion, has already been alluded to when treating of the marine engine.

It is evident that, although there is a limit tolerably well defined in the scale of temperature, below which we cannot expect to pass, a degree gained in approaching this lower limit is more remunerative than a degree gained, in the range of temperature, available by increasing temperatures.*

Hence the attempt made by the French inventor, Dr. Trembly, 25 years ago, and by other inventors since, to utilize a larger proportion of heat by approaching more closely the lower limit, was in accordance with known scientific principles. The form of engine here referred to is known among engineers as the Binary Vapor Engine. In it the heat of the water from the condenser of the steam engine is made to evaporate some very volatile liquid, as ether or carbon-disulphide, which, in turn, by the expansion of its vapor, develops additional mechanical power. Mechanical difficulties have hitherto prevented the success of this form of engine, but it cannot be pronounced unlikely that coming inventors may make the system commercially valuable.

We may now summarize the result of our

* The fact here referred to is easily seen, if it is supposed that an engine is supplied with steam at a temperature of 400 deg. above absolute zero and works it, without waste, down to a temperature of 200 deg. Suppose one inventor to adapt the engine to the use of steam of a range from 500 down to 200 deg., while another works his engine, with equally effective provision against losses, between the limits of 400 and 100 deg.—an equal range with a lower mean. The first case gives an efficiency of one-half, the second three-fifths, and the third three-fourths, the latter giving the highest effect.

examination of the growth of the steam engine thus:

First.—The process of improvement has been one primarily, of differentiation;* the number of parts has been continually increased; while the work of each part has been simplified, a separate organ being appropriated to each process in the cycle of operations.

Secondly.—A kind of secondary process of differentiation has, to some extent, followed the completion of the primary one, in which secondary process one operation is conducted partly in one and partly in another portion of the machine. This is illustrated by the two cylinders of the compound engine and by the duplication noticed in the Binary engine.

Thirdly.—The direction of improvement has been marked by a continual increase of steam pressure, greater expansion, provision for obtaining dry steam, high piston speed, careful protection against loss of heat by conduction or radiation and, in marine engines, by surface condensation.

The direction which improvement seems now to be taking, and the proper direction, as indicated by an examination of the principles of science, as well as by our review of the steps already taken, would seem to be:

EN RESUME—WORKING BETWEEN THE WIDEST ATTAINABLE LIMITS OF TEMPERATURE. Steam must enter the machine at the highest possible temperature, must be protected from waste, and must retain, at the moment before exhaust, the least possible amount of heat. He whose inventive genius, or mechanical skill, contributes to effect either the use of higher steam with safety and without waste, or the reduction of the temperature of discharge, confers a boon upon mankind.

In detail: In the engine, the tendency is, and may probably be expected to continue, in the near future at least, toward higher steam pressure, greater expansion in more than one cylinder, steam jacketing, superheating, a careful use of non-conducting protectors against waste, and the adoption of higher piston speeds.

In the boiler, more complete combustion without excess of air passing thorough the furnace, and more thorough absorption of heat from the furnace gases.

The latter, I am inclined to suppose, will be ultimately effected by the use of a mechanically produced draught, in place of the far more wasteful method of obtaining it by the expenditure of heat in the chimney.

In construction we may anticipate the use of better materials, and more careful workmanship, especially in the boiler, and much improvement in forms and proportions of details.

In management, there is a wide field for improvement, which improvement we may feel assured will rapidly take place, as it has now become well understood that great care, skill and intelligence are important essentials to the economical management of the steam engine, and that they repay, liberally, all of the expense in time and money that are requisite to secure them.

In attempting improvements, in the directions that I have indicated, it would be the height of folly to assume that we have reached a limit in any one of them, or even that we have approached a limit.

If further progress seems checked by inadequate returns for efforts made, in any case, to advance beyond present practice, it becomes the duty of the engineer to detect the cause of such hindrance, and having found it, to remove it.

A few years ago, the movement toward the expansive working of high steam was checked by experiments, seeming to prove positive disadvantage to follow advance beyond a certain point.

A careful revisal of results, however, showed that this was true only with engines built, as was then common, in utter disregard of all the principles involved in such a use of steam, and of the precaution necessary to be taken to insure the gain which science taught us should follow.

The hindrances are mechanical, and it is for the engineer to remove them.

We have seen that the most important problem offered the engineer for solution is a double one, and that it requires the aid of both the scientist and the mechanist in its solution.

But it is sufficiently evident that, before the engineer can determine what form of machine will best yield to him the control of these forces of nature, he must have sufficient knowledge of science to be able to understand what scientific principles are to be rendered available, and what phenomena of nature are operating in the production of the power which he is to seize upon and usefully to apply. Otherwise he will grope in

* This term, though perhaps not familiar to engineers, expresses the idea perfectly.

the dark, and will only learn, by the bitter experience of costly failures, to make slow progress toward perfection.

We have seen that the greatest improvements effected in the steam engine were due to the united engineering skill and experience, and to the scientific attainments of James Watt.

We saw that his improvements followed a long course of intelligent and scientific research, and that directed by the results of this investigation, the engineering talent and the mechanical knowledge of the great inventor, accomplished more in a single lifetime than had been previously accomplished in the whole period embraced in the history of civilization.

This great example confirms what we should infer from the nature of the problem itself, that he who would accomplish most, in the profession of the mechanical engineer, must best combine scientific attainments, especially experimental knowledge, with mechanical tastes and ability and engineering experience.

As one of our oldest engineers* tells him, he must "cultivate a knowledge of physical laws, without which, eminence in the profession can never be securely attained."

He must become familiar, not only with science and the arts, but he must teach himself to make the one assist the other; he must learn just how to make use of scientific principles in planning his work, and how to do his work most thoroughly, efficiently, and economically when he has determined his general design. He must be able to determine how far standard designs are in accordance with correct principles, to detect their defects and to provide a remedy correct in principle and mechanically efficient.

Science and Art must always work hand in hand.

But how is the rising generation of engineers to acquire this proficiency in both branches of knowledge? How are they to be made mentally and manually accomplished; how fitted for the great work which is before them?

The time has gone by when, in any art, the ignorant and merely dexterous workman can compete with even a less skilful shopmate, in the race for preferment, where the latter possesses and uses brains as well as hands, and knows how to make the one direct and aid the other. We, to-day, find him occupying a decided vantage ground who is, at the same time, familiar with the schools and at home in the workshop.

Whatever department in the arts a youth may be designed for, he must, to insure success in the future, be taught not "in the school or the workshop," the alternative formerly offered him, but in the school and the workshop.

Here then arises the necessity of "Technical and Industrial Schools," in which, if properly conducted, knowledge is imparted so as not only to train the mind to habits of thought and study, to give it capacity for logical deduction, but in such a manner as shall, at the same time, make the student familiar with the principles of the art which he is to practise, and prepare him to learn the lessons taught in the workshop and the manufactory, rapidly and well.

It is the tardy recognition of this great want, this vital necessity, that has placed a great nation, which has been far in advance of all others in manufactures and the useful arts, in a position, relatively to her neighbors, that is causing the greatest uneasiness in the minds of the more intelligent of her people and of her statesmen.

They see other nations, who were formerly far behind, now rapidly overtaking her, if not not already taking the lead, in consequence of the earlier adoption of a system of technical education for their people.

Two hundred years ago, Edward Somerset, the second Marquis of Worcester, the inventor, whose work has become familiar to use, informed his fellow-countrymen of the growing necessity for such a system of education for the people, and urged the establishment of technical schools.

For this he deserves higher honor than for anything that he did for the steam engine.

But the system first took a definite form upon the continent of Europe, and, for more than a quarter of a century, it has grown with the growth and strengthened with the strength of the western European nations, until it has, to-day become a most important element of their national power.

In our own country this great need has been long recognized, but the policy of our Government does not permit it to institute systems of teaching, as has been done by those of Europe, and it has remained, to a great degree, unprovided for.

Such education cannot be provided at the small cost that the average citizen can well

*Charles Haswell.

afford to pay, and, even if that were possible it is quite doubtful whether the vital necessity of such an education, to the people rather than to the individual, and to the coming rather than to the present generation, would be sufficiently well understood to induce the payment of its actual cost, far below the real value, as it may be.

It becomes therefore the privilege and the duty of the wealthy among our citizens, to provide this great want of our country, and to aid, thus most effectively, in giving her that pre-eminence among nations that every patriotic citizen aspires to see her attain.

I have referred to the services rendered by John Stevens and his sons in the introduction of the locomotive and the steamboat, but no single achievement of theirs, nor all combined, has conferred so great a benefit upon the country and upon the world, as did the bequest by which one of those sons, Edwin A. Stevens, founded a technical school, where the youth of the present and of succeeding generations are to be taught the principles upon which is based the profession which he and his father and brothers practised with such splendid results.

In St. Paul's Cathedral is a Latin inscription which informs the visitor that no memorial is erected to its great architect, Sir Christopher Wren, but that his most appropriate monument is the great cathedral itself.

So it is here, we see no memorial erected to the founder of this first School of Mechanical Engineering, but "*si monumentum quaeris, circumspice.*"

REPORTS OF ENGINEERS' SOCIETIES.

RAILWAY MECHANICS' ASSOCIATION.—The following is a list of the subjects to be considered at the next meeting of the Master Mechanics' Association, and the list of the different committees to report upon the same:—

1. *Locomotive Boiler Construction.*—S. J. Hayes, Illinois Central; J. Losey, Louisville, New Albany & Chichago; J. B. Gregg, Erie Railway, Committee.

2. *The Operation and Management of Locomotive Boilers, including the Purification of Water.*—H. A. Towne, Hannibal & St. Joseph; A. H. De Clercq, Toledo, Peoria & Warsaw; Harry Elliott, Ohio & Mississippi, Committee.

3. *The Comparative Value of Anthracite Coal, Bituminous Coal and Wood for Generating Steam in Locomotives.*—Chas. Graham, Lackawanna & Bloomsburg; L. S. Young, Cleveland, Columbus, Cincinnati & Indianapolis; B. H. Kidder, Lake Shore & Michigan Southern, Committee.

4. *The Construction, Operation and Cost of Maintaining Continuous Train-brakes.*—J. M. Boon, Pittsburg, Fort Wayne & Chicago; J. Johann, late of Missouri Pacific; W. S. Hudson, Rogers Locomotive Works, Committee.

5. *The Relative Cost of Operating Roads of Gauges of 3 ft. 6 in., or less, and those of the Ordinary 4 ft. 8½ in., Gauge.*—J. T. Robinett, Atlantic, Mississippi & Ohio; J. U. Eastman, Nashville & Chattanooga; W. Bell Smith, South Carolina Railroad, Committee.

6. *The Construction and Operation of Solid-end Connecting Rods for Locomotives.*—J. Sedgley, Lake Shore & Michigan Southern; J. W. Nesbitt, Evansville, Terre Haute & Chicago; N. E. Chapman, Cleveland & Pittsburg, Committee.

7. *Resistance of Trains on Straight and Curved Tracks, and on Wide and Narrow Gauge Roads, and with Four or Six-wheeled Trucks, and with Long and Short Wheel-base.*—W. A. Robinson, Great Western of Canada; Wm. Jackson, Rome, Watertown & Ogdensburg; C. T. Ham, New York Central & Hudson River, Committee.

8. *The Efficiency of Check or Safety-Chains on Engine, Tender and Car Trucks in Lessening the Danger Resulting from Running off the Track.*—R. Wells, Jeffersonville, Madison & Indianapolis; G. R. Peddle, St. Louis, Vandalia, Terre Haute & Indianapolis; J. L. White, Evansville & Grawfordsville, Committee.

9. *Machinery for Removing Snow from the Track.*—J. W. Philbrick, Maine Central; J. N. Foss, Vermont Central; E. Studley, Concord Railroad, Committee.

10. *The Machinery and Appliances for Supplying Fuel and Water to Locomotives.*—H. L. Leach, Hinckley & Williams Locomotive Works; Wilson Eddy, Boston & Albany; E. Garfield, Hartford, Providence & Fishkill, Committee.

11. *The Machinery and Appliances for Removing Wrecks and Erecting Bridges.*—Morris Sellers; D. O. Shaver, Pennsylvania Railroad; S. Moore, Pittsburgh, Fort Wayne & Chicago, Committee.

12. *The Best Form and Proportions of Axles for Cars and Locomotives, also Whether there is Anything to be Gained by the Use of Compound Axles and Loose Wheels.*—M. N. Forney, Colman Sellers, Gordon H. Nott, Committee.

The Committee recommended to the consideration of the Association the advantages which would accrue from offering a premium for the best design and drawing of machinery for accomplishing the removal of wrecks, erecting bridges, and removing snow. To these recommendations of the Committee was added the subject of safety valves, "and the best form and proportions of axles, and what gain in using compound axles, and loose wheels, safety valves and slide valves."

AMERICAN SOCIETY OF CIVIL ENGINEERS.—At the late annual convention, George H. Norman, Esq., of Newport, R. I., proposed to institute a prize fund which will enable the society to award a gold medal yearly for the best essay on engineering topics.

The propriety of changing the method of pub-

lishing the society's transactions was discussed, and their issue in engineering journals advocated. The organization of local clubs or chapters, to be connected with the Society, was considered, and a committee appointed consisting of Messrs. E. S. Chesbrough, of Chicago; O. Chanute, of Kansas City; C. Clarke, of Philadelphia; Horatio Allen and W. Craven, of New York, to report a plan therefor at the next annual convention.

Members of the Society and others connected with public works were asked to send copies of Reports thereon, to the secretary for the Society's library.

Generals W. S. Smith, of Chicago, J. G. Barnard, and G. B. McClellan, of New York, Capt. J. B. Eads, of St. Louis, and A. Fink, Esq., of Louisville, were appointed a committee under the following resolution:

Whereas, American engineers are now mainly dependent upon formulæ for the calculation of strength of the different forms of iron and steel, not based on experiment upon American materials and manufacture, and

Whereas, these differ greatly in many of their characteristics from those of foreign production; both in their nature and forms, therefore

Resolved, That a committee of five be appointed to urge upon the United States Government, the importance of a thorough and complete series of tests of American iron and steel, and the great value of formulæ to be deduced from such experiments.

For the meeting September 4th next, a paper on "Rail Economy," by C. P. Sandberg, of London, and a discussion of Gen. Theo. G. Ellis' published paper on "Experimental Strains in Bowstring Girders," are set down.

THE NEW YORK SOCIETY OF PRACTICAL ENGINEERING.—At the monthly meeting held August 1, Mr. J. F. Cisneros read a paper (a continuation of one on the same subject read at the previous meeting) on Narrow Gauge Railways, taking up the subjects, on this occasion, of depots, bridges, and rolling stock. In specifying the economy of these, as compared with those of common gauge, he claimed in the matter of workshops a saving of 28 per cent., and in engine-houses of 21.71 per cent. The average saving in construction of permanent way and buildings was set down at 24.68 per cent., and the total economy, including that in rolling stock, etc., at from 30 to 35 per cent. of the average cost under the present system. The paper was replete with valuable data and called out much discussion, the opponents of the narrow gauge taking the ground that the same economy could be secured by light rails and rolling stock on the common gauge, or, in other words, that the gauge could be made with truest economy on *light* rather than *narrow* roads.

THE INSTITUTION OF CIVIL ENGINEERS, LONDON.—During the quarter ended on June 30, two hon. members Prince Arthur and Dr. Percy, 8 members and 50 associates have been elected, and 21 students admitted by the council; 23 members have died or resigned, leaving the actual net increase 58. The numbers of the different classes are now as follows:—Hon. members, 16; members, 756; associates, 1,127; and students, 244; making a total of 2,143.

IRON AND STEEL NOTES.

CAN OUR PRODUCTION OF IRON BE INCREASED SUFFICIENTLY TO MEET FUTURE DEMANDS?—The arguments of the advocates of free trade in favor of the abolition of the duties on iron are all exploded by the recent sudden advance in the prices of the article in Europe. If there were no duty now, iron producers in America would receive more for their work than they got a few months ago. There does not, therefore, exist at the present time any question as to the profitableness of this branch of industry, and the only one to be considered is the ability of those now engaged, and those who will engage in it, to supply what is needed.

A recent report of the Secretary of the American Iron and Steel Association presents statistics which will be scanned with much interest. Here is an extract:

The whole production of anthracite pig iron in the United States in 1871 was 957,608 tons, and the growth of this branch of manufacture is shown by the following table:

In 1854	the production was	339,435	tons.	
In 1855	" "	381,866	"	Inc. 12 per cent.
In 1856	" "	443,113	"	Inc. 16 "
In 1857	" "	390,385	"	Dec 12 "
In 1858	" "	361,430	"	Dec 7 "
In 1859	" "	471,745	"	Inc 30 "
In 1860	" "	519,211	"	Inc. 10¼ "
In 1861	" "	409,229	"	Dec 21 "
In 1862	" "	470,315	"	Inc. 15 "
In 1863	" "	577,638	"	Inc 23 "
In 1864	" "	684,018	"	Inc 11 "
In 1865	" "	475,558	"	Dec 30 "
In 1866	" "	749,367	"	Inc. 51 "
In 1867	" "	798,638	"	Inc. 7 "
In 1868	" "	893,000	"	Inc 12 "
In 1869	" "	971,150	"	Inc. 9 "
In 1870	" "	939,000	"	Dec. 4¼ "
In 1871	" "	957,608	"	Inc. 3 "

The present annual capacity of our anthracite blast furnaces, including those nearly ready to be blown in, is about 1,275,000 tons.

As before stated, it is not now possible to give the product of the raw bituminous coal, coke and charcoal furnaces for last year with accuracy; but the very full reports already received from many of the principal iron regions of the country enable us to estimate the production with sufficient precision for practical purposes, as follows:

Raw bituminous coal and coke pig iron ..	570,000 tons.
Charcoal.....	385,000 "

The total product of pig iron in the United States in 1871 may, therefore, be set down at 1,912,608 tons, against 1,865,000 tons in 1870; 1,916,000 tons in 1869; 1,603,000 tons in 1868; 1,461,626 tons in 1867; 1,350,943 tons in 1866, and 931,000 tons in 1865.

The utmost present annual capacity of the blast furnaces of the United States is about 2,500,000 tons.

The total production of rails in the country in 1871 amounted to 775,733 tons, as compared with 620,000 tons in 1870; 593,586 tons in 1869; 506,714 tons in 1868; 462,108 tons in 1867; 430,778 tons in 1866, and 356,292 tons in 1865. Of the product of 1871, 715,691 tons were iron and 60,042 tons steel and steel-headed. The following state-

ment shows the localities in which these rails were manufactured:

Pennsylvania	335,604 tons.
Illinois	91,178 "
New York	87,022 "
Ohio	75,782 "
Maryland	44,941 "
Wisconsin	28,774 "
Massachusetts	28,864 "
Michigan	14,000 "
Maine	13,383 "
Indiana	12,778 "
Tennessee	9,667 "
Missouri	8,260 "
New Jersey	6,700 "
Georgia	7,840 "
West Virginia	5,000 "
Kentucky	6,000 "

The reports received from the rail mills indicate that a large number were in operation during a part of last year only. There are forty-eight rail mills in the country, and their utmost annual capacity is placed at about 1,000,000 tons.

The product of rolled and hammered iron other than rails in 1871 is estimated at 700,000 tons, which may be analyzed about as follows:

Merchant bar and rod	335,000
Sheet and plate	118,000
Hoop	22,000
Nails and spikes	155,000
Axles and other	80,000
Total	710,000

The product of steel of all kinds in 1871 is estimated at 82,000 tons, of which 45,000 tons were made by the Bessemer process.

The amount of pig iron produced in 1871 was not greater than that of 1869, and the importation of rails in 1871 from Great Britain amounted to 572,386 tons, in addition to 210,687 tons of pig iron. Nearly half of the rails consumed in the last year came from that country. The cost of iron importations for the last fiscal year, if the Treasury report be reliable, is presented in the following figures:

The declared dutiable value of iron and manufactures thereof imported in the year ending June 30, 1871	$31,852 034 83
And of steel and manufactures thereof	11,404,084 85
Total	$43,256,119 68

Since the close of the fiscal year importations have largely increased, and the great advance in prices will so swell the amount which will be received during the current year that it will require very nearly if not quite the whole product of the gold mines of the United States to pay for it. This is not an encouraging view of the trade in metals for the advocates of a resumption of specie payments.

Since our country has had a separate existence the matter of protective and fostering laws to encourage native industry has been in controversy, and the struggle between opposing parties appears to be as far from an end as it ever was. Is it not strange that in this age of wonderful enlightenment so intelligent a people as those of the United States cannot find out which policy is best for the promotion of the general prosperity of the country? A little cool observation, however, will show that selfish interests more than a lack of understanding influence the opinions of antagonists. Those who have their capital invested in iron furnaces advocate protective duties, and those who are engaged in the business of importing foreign products clamor for free trade. At the present time, however, it is manifest that no tariff of duties can remedy the want of a supply of iron. The high prices come from abroad, and it is evident that a sufficient supply at moderate prices cannot be expected for some years to come. Other means besides tinkering tariff laws must be adopted, and it can not be doubted that one of them is to give practical encouragement to the production of steel rails. Their greater durability will relieve the labor of the country of the necessity of frequently relaying tracks and re-rolling rails, and a greater amount of labor may be given to producing the metal. Great improvements in the manufacture of iron and steel have been made, and invention will be stimulated by the high prices which will prevail. Increased cost is a disadvantage, because it will augment our foreign debt; but it will encourage home industry, and if the Government would furnish a currency which would make capital as cheap as it is in Europe, this department of production might now be placed upon a footing which would enable it to defy competition. One hundred millions in greenbacks annually added to the available means of the business community would give an impetus to railroad and steamship enterprises, which would augment the wealth of the country in an incalculable degree; but it requires brains to enable politicians to see it.

THE IRON TRADE.—The last bulletin of the "Comités des Forges" published the statements of iron exports and imports during the first three months of 1872 and the corresponding period of 1870. The results are as follows:

	Three months, 1872.		Three months, 1870.	
	Cast iron.	Wrought Iron.	Cast Iron.	Wrought iron.
	tons.	tons.	tons.	tons.
Imports ..	32,317.819	15,257.210	35,255 .9 :1	19,018.135
Exports ..	124,658.049		100,704 109	

The quantities of iron ores brought into France during the same periods are respectively 106,355,847 for 1872, 117,915,610 tons for 1870. The German importations have suffered a great diminution; those of Spain, on the contrary, have increased by half, and the importation of Algerian ores also rises.

In 1870 the production of the French mines fell to 12,000 tons, whilst it was 14,000 tons in 1869. At the end of 1871 activity was re-established, and the production exceeded 13,000 tons; in fact, the coal yield has not sensibly diminished, despite the fact that the coal basin of the Moselle has been lost to the country.

RAILWAY NOTES.

NEW NARROW GAUGE ROAD.—Mr. Joseph Klieberg, writing from Brownsville, Texas, says: We are building a narrow gauge road, viz. 3 ft. 6 in., length of road, from Point Isabel to this place, 22 miles. 17 miles graded and ties on track now. 2 locomotives, built by M. Baird & Co., are set up and ready, also 20 cars built by Jackson, Sharpe & Co., also set up, all at Point Isabel. The rails are expected to arrive in a few days. Ship sailed from Cardiff on April 10th last. We have also built a Dock and Depot at Point Isabel. Consequently the road ought to be in running order in a short time, and it will not be long before we will have a practical demonstration of the advantages and usefulness of a Narrow Gauge Road in this part of the world. If our road proves all that is claimed for this gauge, our example will be of immense value to the people of Texas, as the cost so far has been 35 per cent. less than the cost of the old wide gauge now in use in this State.

It is principally owing to the valuable and interesting articles on Narrow Gauge published in your Magazine that this road has been built.

The railroad laws of Texas require the wide gauge. I went to Austin in February, 1871, to get the Legislature to pass a special act to allow us the narrow. I was laughed at and met with serious opposition. When a friend of your brother Prof. Van Nostrand told me that if I would refer some of those gentlemen to the Professor that they might become convinced of the practicability of the narrow gauge standard. I did so and got my bill passed. And now the road is an assured success, at least as far as the construction is concerned.

THE CENTRAL PACIFIC ROUTE.—There has been considerable speculation with regard to the reasons which caused the managers of the Pennsylvania Company to allow the control of the great central route to the Pacific to be taken from them. The change was spoken of by some as a victory of Vanderbilt's; but it was evident that the Pennsylvania Company made no effort to continue their management of the concern. Quite a number of suggestions have been made as to what was most probably *not* the reason for this lack of effort on the part of so active and enterprising an institution, and a contemporary expresses the belief that the main reason was the desire to operate the Northern Pacific road, whose terminus will be so much nearer to Japan and China as to receive the bulk of the trade coming from that part of the world. We have heretofore given a view of the matter, which is, that the Pennsylvania colossus perceives the great advantages of the Southern route, and will push it forward to completion in the shortest time practicable.

The Northern Pacific road is progressing favorably, and as a summer route it will have many advantages; but it is located so far north as to make it expensive and difficult to work in winter. The southern route will not only avoid the objectionable features of the central and northern ones, but it will occupy a part of the country which will be rapidly developed, and the southern branches will bring an immense trade to it.

We should not shut our eyes to the progress of events, nor suppose that the commerce of the continent will always follow the same course which it takes now. It is not likely that a country like Mexico will be allowed much longer to remain in the hands of the barbarians who have heretofore mismanaged its affairs, and prevented the development of its resources; and a careful study of the map will show that the most favorable route from the Mississippi valley to the Atlantic is below instead of above the latitude of St. Louis. It is not improbable that a city in the vicinity of Hampton Roads, if it be not Norfolk, will one day be the chief commercial port of the country, and the leading railroad will run nearly due west from that point. The Southwestern States have very great resources, which will be brought forth rapidly when more favorable circumstances lend their aid, and a more sensible financial policy than that which is now grinding the country and checking enterprise shall have been introduced.

Time will show that the shrewd managers of the Pennsylvania Railroad Company were not ambitious to feed the elephant which they had got into their possession, and that they willingly yielded that honor to a rival, in order to devote their time, talents and means to securing what will be in time much the most important route across the continent.

ENGINEERING STRUCTURES.

THE BERMUDA FLOATING DOCK.—The following article appeared in the "Bermuda Royal Gazette" of the 20th ult.:—"A brief description of the operation of placing in the floating dock the large iron plate man-of-war Royal Alfred, bearing the flag of Admiral Fanshawe, may interest some of our readers. Other large ships of war have been safely and successfully received into the same capacious bosom, but none nearly so large as the Royal Alfred, which weighs about 6,000 tons; and the docking of a first-class man-of-war of this kind has been looked forward to as the crucial test. The floating dock has stood this test well, verifying in every particular the scientific calculations of its projectors. Casual visitors to the dockyard during the past year may have observed large iron bridges growing plate by plate until like ships 120 ft. long. They weighed about 50 tons each, and were run out on rollers with ease. One end was attached to a strong weighted platform on shore, the other end supported on lighters ready to be hooked on the dock so as to rise and fall with it. Two of these bridges are hooked on to the side and one to the end of the dock, keeping it steady in its place; and from these gangways or bridges arise strong commodious ladders, by which workmen can speedily and safely reach all parts of the great structure. On Saturday, the 11th of May, the ship was moved across the Camber, and on Tuesday, the 14th, when everything was ready, the ship was placed in the rear of the dock, pointing fairly towards its central line. Steam capstans in the dock assisted the ship's capstans. There were two hawsers over her bows, one over each quarter, one right ahead, and a checking hawser right astern. Each of these was diligently and skilfully attended to by competent officers, and, thus tenderly guided, she moved in majestically, her keel being exactly over the central line of blocks ranged along

the bottom of the dock, on which her huge bottom was presently to rest. The dock was then allowed to rise enough to touch gently first one end and then the other of the ship's keel, with just sufficient pressure to prevent the least lateral motion. The ship at this moment was drawing 25 ft. Strong timber shores were fixed all round the top sides. The dock was then allowed to rise gradually about 10 ft. The caissons were placed in position, the ship being in 15 ft. water. The work of shoring up followed the subsiding of the water, which was allowed to run into the bottom of the dock, so as to carefully counterpoise the whole load, keeping exactly level. A close examination of the ship showed that the whole of the false keel was gone, bolts and all cut clean away. It had served the purpose for which it was put on by protecting the ship's keel from abrasion. The lower part of the bilge pieces were much rubbed, small patches of copper, broken away in parts, and some galvanic destruction of the Muntz's metal was apparent where the rust of the iron plating drained through the sheathing of wood; the copper was remarkably clean, and the planking perfectly sound and good. The quiet manner in which this important operation was performed, without the slightest accident, confusion, or interruption, reflects the highest credit on all concerned in it, particularly Mr. Barnaby, who came out in the dock nearly three years ago, and has directed all works which have led to this triumph of mind over matter, giving complete effect to the wonderful design in which the necessary forces are borrowed from nature, on the true hydrostatic principle of making the water to work for instead of against. There was no pumping; the fluid was simply sent where its weight was wanted, and removed from places where it was superfluous.

BOOK NOTICES.

REPORT ON THE FABRICATION OF IRON for Defensive Purposes and its Uses in Modern Fortifications, especially in Works of Coast Defence. Washington: Government Printing Office.

This is one of the later professional papers of the Corps of Engineers of the United States Army.

In June, 1870, Brevet Major-General Barnard, Colonel, and H. G. Wright, Lieut.-Colonel of the Corps of Engineers, and Brevet Lieut.-Col P. S. Michie, U. S. Eng., were directed, by order of the Secretary of War, to proceed to England, France, Belgium, Prussia and Russia, for the purpose of collecting information upon the subject fully set forth in the above title.

The report of this Commission is embodied in a handsome quarto of 230 pages, and largely illustrated by folding plates and cuts in the text.

A brief glance at the leading features of the report will serve to convey an idea of the nature of the labor performed by the Commission, and its value and interest not only to military men, but to scientific readers everywhere.

The problems which military engineers have presented for solution are in general so widely different from those that claim the attention of their *civil* brethren, that there seem to be but few topics of interest common to both orders of the profession. In past times both used, to be sure, the same or similar materials in solving their practical problems; but for widely different purposes, and to provide for the resistance of forces so different in their action that no common measure of them was suggested to the average scientific mind.

The masonry designed to receive the impact of a cannon ball, as in a fort, and the masonry which was built to resist the wear and push of a running stream, might with propriety be of the same rock material, but it is evident that the problems presented in the two cases are so unlike that there seem to be no data belonging equally to both.

In these later times, however, when a single material is being employed for such widely different purposes, and when, moreover, the railway, with its rectangular truss bridge, has become an important aid to army movements; when, on the other hand, pile foundations are driven by gunpowder, it would seem as though the time were approaching in which the labor of the Military and Civil Engineer would differ only in the final objects to be attained, and not in the process of accomplishing them. The ground on which they work in common is widening daily. In the manufacture and use of iron for whatever purpose, engineers everywhere now feel a vital interest, and all experiments which tend to add to our knowledge of the capabilities of this precious metal to resist forces of any kind, are regarded with equal interest by artillerist and bridge builder.

The most cursory glance at the work before us assures us that the labor of the Commission was most thoroughly done, and the record of their labor was under the direction of a master in scientific investigation.

The Commission began their investigation with English offensive and defensive structures. The questions which had vexed the military mind in that country when first adopting iron in the construction of their defences is thus set forth in the early pages of the report:

"The great modifications which these changes must introduce in the construction of sea-coast batteries was not foreseen. Nevertheless, before any considerable progress in its actual execution could be made, the necessity of modifications became obvious. That iron must be substituted for masonry wherever, as in the walls of casements, it had been customary to expose the latter to the direct fire of ships' guns, was at once—perhaps too readily—admitted. What the character of iron construction should be was far less easily arrived at. Should it be of bars or beams or of plates? If of the latter, should the shield or covering mass be made up of one or more very thick plates, or of numerous thin ones? Should these be rolled or hammered? How should they be fastened to each other and their supports? Should masonry be excluded entirely from the fronts of casemated batteries, or might it not still be employed in combination with the iron? While the English have the advantage of having all these questions started before their structures were so much advanced as to be incapable of the requisite modifications, yet, on the other hand, they have the disadvantage of being compelled to go on with construction without waiting for their final solution."

Besides the description of the forts and their equipments, which occupies considerable space in the report, the process of making and testing armor plates and bolts is particularly valuable as well as interesting.

Of course it was to be expected that in England the Commission would find the works that would occupy a large share of their attention; and it is in this part of the work consequently that the description involves the details of the metallurgical processes, as well as other processes and implements employed in the manufacture of iron armor and iron and steel guns.

In Prussia the Commission found that no direct application of iron to harbor defence had yet been made. Some plans of arranging and protecting casemates had been projected, and our party witnessed the results of some tests to which the proposed improvements had been subjected. Several full-page plates illustrate this portion of the report.

In Austria nothing described bears directly upon the use of iron in fortification. The construction of the works at Pola is briefly explained, and the fact recorded that the Austrian engineers as yet "use no iron in their defences, considering it too expensive, and that the results thus far developed by experiment in various countries are not altogether satisfactory."

In Belgium the extent of iron construction in fortifications is limited to a light turret upon a land fort. The general plan of the defences is, however, described and illustrated by maps.

The construction of torpedoes forms the subject of a chapter. Artillery naturally occupies a large space in the record of the labors of the Commission. The "battle of the guns, as a recent writer termed it, was already begun, and indeed is yet being waged. Its stage of progress two years ago is not completely recorded so far as text and plate can do it.

A chapter on "Naval Defensive Construction" is devoted to a detailed description of the vessels designed especially for defence of coasts.

"General Remarks on Fortification" closes the report proper. This chapter furnishes quite a complete treatise on fortifications, containing besides, a description of the system now in course of construction in Belgium.

The appendix, or rather appendices to the report, for there are eleven different sections, bear titles as follows:

I. Manufacture of Broad Armor Plates.

II. Iron Concrete Backing.

III. Memoranda on the Condition of Magazines and other buildings covered with earth, in respect to dampness.

IV. Record of Experiments at Gilliskie Battery, 1868–69.

V. Memoranda of Experiments on the Accuracy and Rapidity of Fire from Heavy Guns against Moving Objects.

VI. Report of the Experimental Firing against Gruson Casemate.

VII. Resume of the Prussian Report on the Experimental Firing against Armor Construction in England, 1868.

VIII. Muzzle Pivoting Carriages.

IX. The 35-Ton Gun.

X. The Harvey Torpedo.

XI. The Progress that has been made in the Application of the Moncrieff system to Garrison, Siege, and Naval Ordnance, and to Coast Works.

These separate chapters are of materials gathered from various sources, and serve to show how wide a range of subjects were necessarily considered in fulfilling the instructions of the War Department, and indicate, moreover, quite as clearly how wide a range of professional knowledge was represented in this numerically small Commission.

We recognize throughout the report the handiwork of the senior officer of the Commission, Maj-Gen. Barnard. One of the most prominent among military engineers in America, he is no less a civil engineer of the highest culture. There is no theoretical investigation so learned and labored but that he follows it with interest and delight when it affords information on the subject he is pursuing, and as, this report indicates, there are no minutiæ of detail in construction so small but that he observes and takes them into account.

SPECTRUM ANALYSIS IN ITS APPLICATION TO TERRESTRIAL SUBSTANCES, AND THE PHYSICAL CONSTITUTION OF THE HEAVENLY BODIES. Familiarly explained by Dr. H. SCHELLEN. Translated from the second London edition by JANE & CAROLINE LASSELL, and edited with Notes by WILLIAM HUGGINS, F. R. S., etc. London, 1872: Longmans. For sale by Van Nostrand.

Any discovery in science has a multiform value, and its bearings upon the various classes of men and upon the world at large are many and widely discrepant. Each class is enabled by its nature to get but one special kind of improvement from any given achievement in the scientific world, and as use is the ultimate aim of scientific thought, it is a matter for congratulation that the leading researches of the present day reach so large an audience.

The subject of light is one peculiarly adapted to impress the very largest class of persons, since an extensive knowledge of its phenomena and properties may be had by experimental means alone, and since such experiments are always striking and brilliant. The purely mathematical treatment of light or heat or sound will be, by its nature, repulsive to the majority of men, and, though of the highest value educationally, is still not of the widest. An instance of this fact is to be found in the effect of the discovery of the planet Neptune, by Adams & Le Verrier. No scientific fact was more widely known and none was more wondered at; but the value of the discovery was confined to a very small body of scientists who were able to follow the solution of the problem of "inverse perturbations," while, as we said, to the mass of men, the effect produced was of wonder merely. Now, wonder and admiration are not supremely valuable as means of education; and it is just for this reason that the appreciation of the higher researches in astronomy must ever be confined to those who can read with intelligence the grand memoirs of Lagrange and La Place, of Hamilton and Pierce. But "the new analysis," as its friends are fond of calling it—spectrum analysis—contains no result, however grand, which may not be reached with a little attention by any one, and its chief value as a means of education is largely due to this fact.

Although the spectrum analysis is of comparatively recent date (1860, yet it has already a large literature, and this is put in an easily accessible form.

"Roscoe's Spectrum Analysis" (New York, 1869: D. Appleton & Co.) states the principal

features of its application to terrestrial substances very clearly, and the logic of Kirchhoff's hypothesis as to the constitution of the sun is admirably set forth. The elements, too, of celestial spectroscopy are there given in the form of extracts from original papers by Huggins and Miller. But such has been the growth of this branch of knowledge that since 1869 almost a new department has been added, viz., the constitution and theory of the solar protuberances. The results of this study are given with exactness by Prof. Young of Dartmouth College, in a small pamphlet on "The Sun and the Phenomena of its Atmosphere." (New Haven; Chatfield, 1872.) The methods by which the wonderful results were reached have been hidden in the pages of proceedings of scientific societies until the publication of this manual by Schellen, the English translation of which renders it easy even to those possessed of no apparatus to follow the most elevated thoughts of spectroscopists. Its title sufficiently indicates its general scope; but even a scientist will be suprised to find the vast number of useful facts and processes which are there given. It would be easy to step from an attentive reading of this book to an intimate practical knowledge of the working of the spectroscope; so admirable are its explanations of processes and so full are its drawings of the various instruments and parts of instruments. The discussion of the subject of the hydrogen atmosphere about the sun is peculiarly clear, and the fine colored plates of its varied forms as observed by Respighi and Young, ought to make this very delicate observation intelligible even to those who have never used the spectroscope. There is wanting in this book, however, that beautiful sequence which makes the essays of some of our modern scientists so delightful even when considered from their literary side alone. Most of the theory is clear, and expresses what the author desires to convey, though in many cases he differs with the highest authorities. The notes of the editor, Dr. Huggins, are usually very valuable, though here and there there occur oversights which might well be corrected. On page 125 there is noticed a form of indicator for registering the position of the lines during an eclipse or at any other time, and the credit of devising the form seems to be given to the optician Grubb. A very brief account of Prof. Winlock's similar method of registering these lines is given; but it would seem to have been more explicit to have stated that Prof. Winlock's plan was described in England by Prof. Pierce some time before the construction of Grubb's apparatus. Again, on page 135, in the description of the automatic spectroscope the fact that Browning's form of it is due to Mr. Rutherford, of New York, is not definitely stated, although a reference is made to the latter astronomer, which would lead us to imagine the Browning form an improvement upon his. On page 406 a mention is made of lines perpendicular to the Frauenhofer lines which occur occasionally in the neighborhood of a solar spot. Such lines are so readily formed by dust on the edges of the slit that it is difficult to believe them to have had any other origin. We have not space to refer in detail to the account given by our author of the corona spectrum, of eclipses, of cometic, and nebulous spectra; but we may say that no more complete description is in print, and that the book is really one which no investigator can afford to be without, while to those in search of general knowledge upon the subject it is simply invaluable. Perhaps it is least full upon the matter of terrestrial spectroscopy; but this is the less to be felt, as the work of Roscoe (mentioned above) amply supplies its deficiencies. E. S. H.

THE PRINCIPLES AND PRACTICE OF CANAL AND RIVER ENGINEERING. By DAVID STEVENSON, F.R.S.E. Edinburgh: A. & C. Black. 1872. For sale by Van Nostrand.

To those interested in river navigation we do not know that we can point out a better source of information than the second edition of Mr. David Stevenson s treatise on "Canal and River Engineering.'

As to inland and ordinary canal engineering, the book is meagre and poor in instruction, but upon the question of river improvements it is rich in information, derived, apparently, from personal experience, and given tentatively and modestly.

The author divides rivers into three compartments: 1, (going upwards from the sea) there is the *sea proper* compartment; 2, the *tidal compartment;* and 3, the *river proper.* "These three compartments possess very different physical characteristics. The presence of unimpaired tidal phenomena in the lowest; the modified flow of the tide (produced by the inclination of the river s bed) in the intermediate; and the absence of all tidal influence in the highest compartment."

Ship canals he separates into three classes, and considers each. They are—1. Those which on their route from sea to sea traverse high districts, surmounting the elevation by locks supplied by natural lakes or artificial reservoirs, such as the Languedoc Canal in France, and the Caledonian Canal. 2. Those in low-lying districts, which are carried on a uniform water-level from end to end, and are defended against the inroad of the sea at high-water by double-acting locks, which also retain the canal-water at low tide, such as the canals of Holland and other low-lying countries. 3. Canals, of which the Suez is the only example yet made without locks at either end, and communicating freely with the sea, from which it derives its water supply.—*Civil Engineer's Journal.*

A POCKET DICTIONARY OF TECHNICAL TERMS USED IN ARTS AND MANUFACTURES. Abridged from the Technological Dictionary of RUMPF, MOTHES and UNVERZAGT, with the addition of Commercial Terms. Three volumes, German-English-French, English-German-French, and French-German-English. New York: For sale by Van Nostrand. Price for the three volumes, unbound, $3 25.

Every engineer, technologist, scientific inquirer or practical mechanic or manufacturer who attempts to follow the literature of his business in other languages than his own, is perpetually embarrassed by the inadequacies of ordinary dictionaries for the translation of technical terms. Such works as the one before us are indispensable to the student who means to go deeper than the surface, and to keep pace with the progress of foreign countries. This dictionary we have found very satisfactory in contents and plan. It is printed in fine but clear and new type, and its price is surprisingly low. The volumes are small quartos in form, and about 1¼ inches each in thickness.

INSTRUMENT FOR ASCERTAINING THE CONDUCTIVITY OF MERCURY.

Constructed by CAPTAIN JOHN ERICSSON.

PAGE 337.

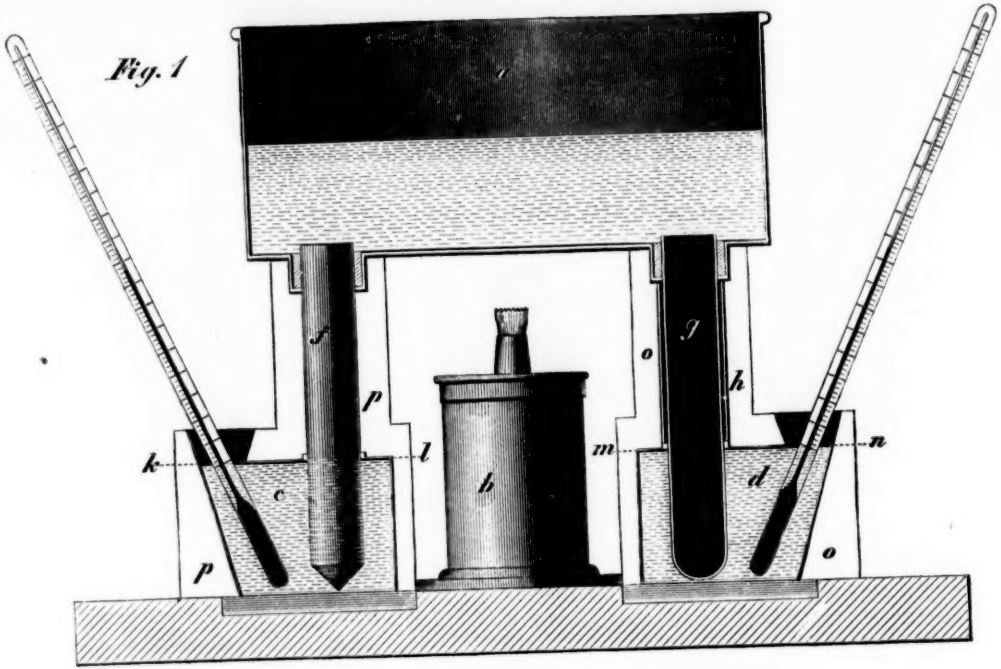



Fig. 1